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VOL. LXX NO. 4 APRIL 1950

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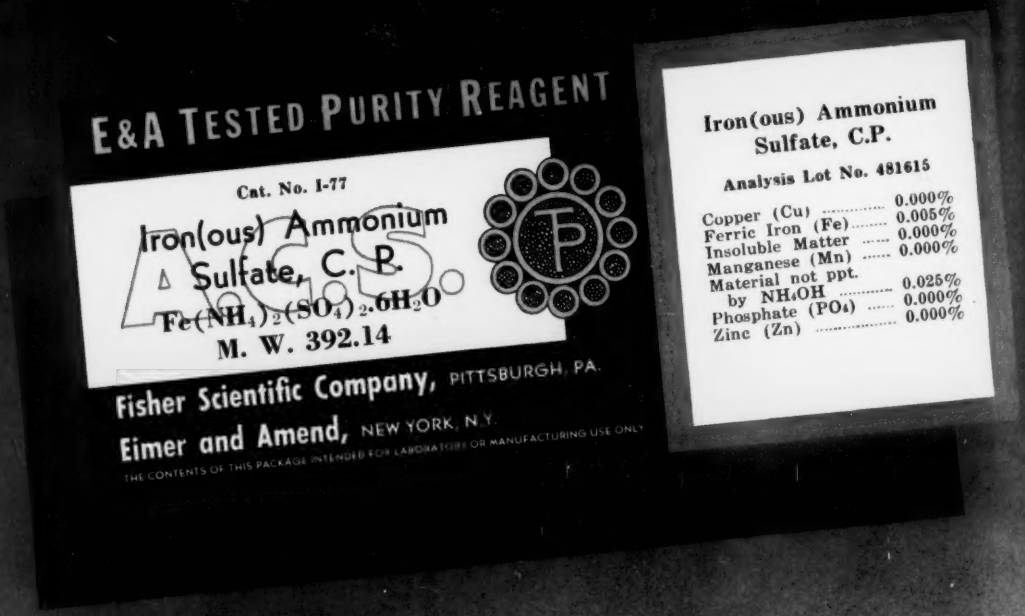
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Science and Technology

(From the Month's News Releases)

Potatoes

New standards of potato grading developed by the Production and Marketing Administration of the USDA will provide tomorrow's housewife with potatoes of better quality and of even-running sizes: Small, Medium, Medium to Large, and Large. Closer sizing will add slightly to retail costs, but there will be less waste from defective potatoes. Adoption of the new grades is optional with shippers.

Anti-T.B.

According to a new law, all French school children, all infants in public institutions, all infants exposed to tuberculosis, all premedical, medical, dental, nursing, and social work students, all personnel in public institutions, all hospital employees, all soldiers and sailors, and all workers in the food trades must be vaccinated with the B.C.G. anti-T.B. serum.

Aeronautics

Test flights by autopilot of aircraft still on the drawing boards are now possible with the new Bureau of Ordnance calculating machine built for the Navy by MIT. The "model"—impulses set as factors on the machine—will clearly show design defects before construction is begun.

General Electric engineers have introduced a "contour-following system," by means of which an electric eye scans a drawing through a microscope. Movements of the eye and microscope are communicated electrically to a machine tool cutting out the part. The system is expected to be of special value in making master patterns for the manufacture of gas-turbine "buckets" or blades for jet engines, machining of which usually requires a great deal of time and extraordinary skill.

The French have successfully tested a jet-propelled helicopter, the S. O. 1100, of very simple design. Its rotor is driven by the reaction to jets discharged from nozzles below each blade. Since a motor is used only to compress the air that is forced into the ignition chambers, the rotor is completely independent of the motor, thus eliminating heavy transmission equipment.

Uranium

Techniques of airborne prospecting for uranium are being developed by the USGS in cooperation with the AEC and are based on prior development by the Survey of aeromagnetic surveying. Background noise due to cosmic radiation is cut down by means of a thick lead shield above the Geiger counters and a special arrangement of counter tubes in hexagonal bundles directly below the shield. Surface prospecting has been speeded up by using shock-mounted Geiger counters in light trucks or automobiles. Subsurface prospecting is carried on by gamma-ray logging of exploratory drill holes.

The Su-5 Beta Gamma Survey Meter is said by its manufacturers to serve the dual purpose of a radiation dosage rate meter and a monitoring instrument. It is portable, battery-operated, weatherproof, and provided with both a handle and a shoulder carrying strap. Because it is equipped with two sets of scale ranges, readings can be made in terms of both milliroentgens per hour and counts per minute.

For private prospectors, the best available guide is the 30-cent handbook *Prospecting for Uranium*. Obtainable from GPO, Washington 25, D. C.

Musicolor

Designed to cover a multitude of commercial uses, Musicolor, which is an animated color lighting system synchronized with music, may also have therapeutic applications in hospitals and sanitariums. The color combinations can be preset, changed at will, and shown on any type of surface, or through plastic screens.

Fire Protection

Factory Mutual Engineering Division, 184 High St., Boston 10, Massachusetts, has recently published a 24-page booklet on home prevention of fires. Free on request, it describes in detail the ways in which home fires can start and how they can be prevented. Specific recommendations are made for the selection of types of extinguishers for home use.

In a series of tests on compounds of the bromofluorocarbon group, the Engineer Research and Development Laboratories at Fort Belvoir, Va., have discovered fire extinguishers twice as effective as ordinary agents such as carbon dioxide and carbon tetrachloride. Toxic effects have not yet been determined.

International Morse Products offers low-cost fire protection in a system designed specifically for home use. Small detector units are placed in a strategic position in each room, the average five-room house requiring 13 units. If the temperature rises above 150–160 degrees, an eutetic metal link breaks, creating an electrical circuit and sounding the alarm. A built-in transformer brings the regular 110-volt system down to 24 volts, making the Morse apparatus very easy to install. Wiring is located between the walls and in the case of fire within the walls, the alarm goes off automatically.

Education

The 1950 foreign agricultural student training program will bring a record number of about 250 agricultural leaders to the U. S. as in-service trainees from more than a score of foreign countries. Arrangements have been made for them to receive specialized training in bureaus of the USDA in laboratories, field stations, and projects and at state colleges and experiment stations. Students have already arrived from Germany, Austria, and Japan and will be followed in a few weeks by a group from France.

Three five-week instructor training courses in radiological monitoring techniques for qualified educators and technicians to be selected by governors of the states, will be offered by the AEC in cooperation with the National Security Resources Board and the GSA. This is part of the program for state and municipal civil defense activity.

Under the general direction of Professor Francis W. Sears, MIT will provide a review of fundamental sciences, as well as a survey of recent scientific advances, to 50 high- and preparatory-school teachers of science next summer. The fellowship program is made possible by a grant of $62,500 from the Westinghouse Educational Foundation.

Soil Conservation

Near Watkinsville, Ga., SCS technicians have been experimenting for eight years on badly eroded lands that formerly failed to produce paying crops. Various grasses, grains, and legumes furnish year-round grazing for profitable beef animals, and prevent further erosion.

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APRIL 1950

Polyploidy in Fruit Improvement

GEORGE M. DARROW

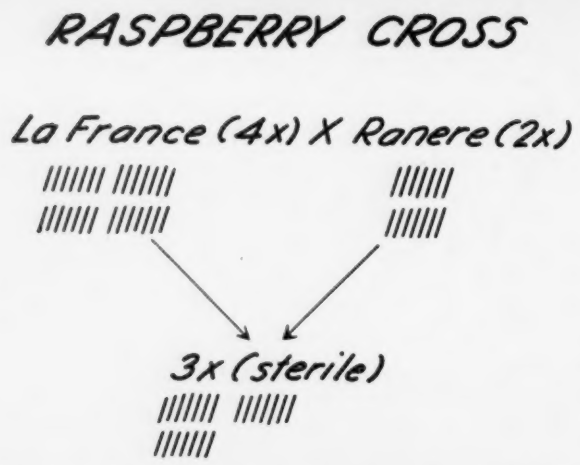
The author (Ph.D., Johns Hopkins, 1927), who is well known for his work in fruit breeding and in the genetics and polyploidy of fruit, is principal horticulturist at the USDA's famous Beltsville, Maryland, Plant Industry Station. His article is based on his presidential address at the annual meeting of the American Society for Horticultural Sciences, in Milwaukee last October.

IN THE early 1920s certain little-grown red raspberry varieties which I was then using in breeding had very stout canes, with notably broad and thick leaves. Their leaves seemed more resistant to disease than many related varieties then widely cultivated. This character made them desirable for parental stock. When crossed with the more common varieties, however, the seedlings produced were relatively sterile. In 1923, Longley found that the common varieties of raspberry all had 14 chromosomes in each of their somatic or vegetative cells; that is, they had 2 sets of 7 chromosomes and therefore were diploids. On further examination he discovered that the stout-caned, broad, and thick-leaved group had 4 sets, or 28 chromosomes per somatic cell, and that the hybrids between these and the common varieties had 3 sets, or 21 chromosomes, per somatic cell. Both of these latter types are classed as polyploids—forms in which 3 or more basic sets of chromosomes occur in the somatic cells. The one with 4 sets is called a tetraploid; that with 3 sets is classed as a triploid. It was further found that the relative sterility of the triploid hybrids was a result of an unbalanced chromosome system. Thus, over a quarter of a century ago I was forced to take polyploidy into account in breeding programs—and it has been an ever-present factor in research on the fruits with which I have since worked.

Having found such a condition in raspberry, Longley made a chromosome survey of the strawberry, where similar problems had appeared. In that fruit the native wild woodland strawberry (*Fragaria vesca* var. *americana*) was found to be diploid, an open-woodland wild strawberry of central Europe (*F. moschata*) was found to be hexaploid, and our common wild meadow strawberry (*F. virginiana*) and all cultivated garden strawberries were octaploid, with 8 sets of chromosomes.

Longley had previously surveyed the blackberries and found a polyploid series ranging from 2 to 12 sets of chromosomes. He also surveyed the blueberry group for F. V. Coville and found three groups: diploid, tetraploid, and hexaploid.

In the raspberry, the tetraploids have strong stocky canes, with large thrifty leaves, in comparison with the diploid. In the strawberry, the hexaploid is sturdier than the diploid, and the octaploid stronger than the hexaploid. In the blueberry, diploids of the same species-group are generally somewhat less vigorous than the tetraploids or hexaploids. In the blackberry there are strong-growing species in each chromosome group, some of the diploids having about the same vigor as some of the 12-ploid group. But it is plainly evident that the 12-ploid blackberries bear a very different relationship to the diploid blackberries from that of the tetraploid raspberries or blueberries to their respective diploids. A true understanding of the effect of polyploidy and the use of polyploid material in



breeding in any group must be based on a knowledge of the relationship of the polyploids to the lower-chromosome species and forms in that group.

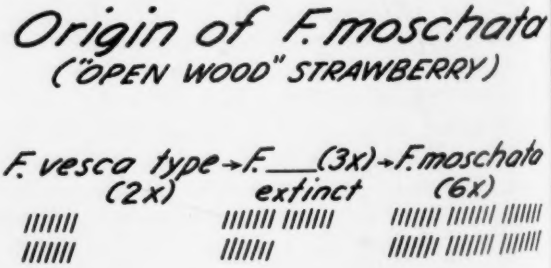
The story of the evolution of the groups from which our small fruits have been derived is shrouded in the mists of the geological past. A botanical friend of mine, particularly interested in such things, informs me that each year evidence is being uncovered indicating that flowering plants were present much longer ago than we have been suspecting. There is excellent evidence that, could we have walked about in the land a million years ago, we would have found many plants much as they are today. There would have been quite recognizable strawberries; various blackberries would have been present, among them the direct ancestors of our present Western trailing sorts, as well as the Eastern forms; and the blueberries already would have been sorted out into various low-bush and highbush kinds.

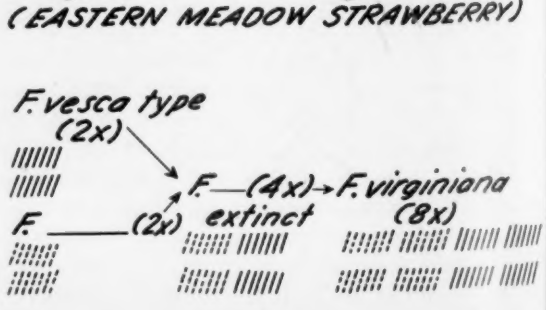
There is little question that some of these strawberries, blackberries, and blueberries of a million years ago were almost identical with some now living in our woodlands and meadows, and near moist seepages and streams; yet could we compare those with the living we also would be able to detect another series of changes. Some of these changes have been very slow—the gradual evolution one might expect over a long period of time. But had we been present throughout the period and able to watch these changes, it also would have been evident that evolution did not always proceed at a steady pace, for outside factors often influence and accelerate the development of new and different sorts. There are clear indications that, in the blackberries, there has been a fairly recent and extremely rapid development of new forms—even within the past two hundred years; the blueberries also have been evolving for a very long time, but certain types seem to have appeared only within the past century, and there is incontrovertible evidence that newer forms are currently being evolved in the wild. The majority of these are polyploids.

A review of what has happened and is happening in these fruits is especially timely because of Waldo's work on the origin of the Logan and Mammoth blackberries reported to the American Society for Horticultural Science two years ago; because of the report on "giant" apples at the same meeting; and because of the recent work of Bain and Dermen in producing tetraploid cranberries. The methods of nature through the ages are beginning to be better understood, and methods are being used to forward fruit improvement (for man's use) so that as much can now be accomplished in breeding plots in a few years as by chance in a hundred thousand or a million years under the slow hand of time.

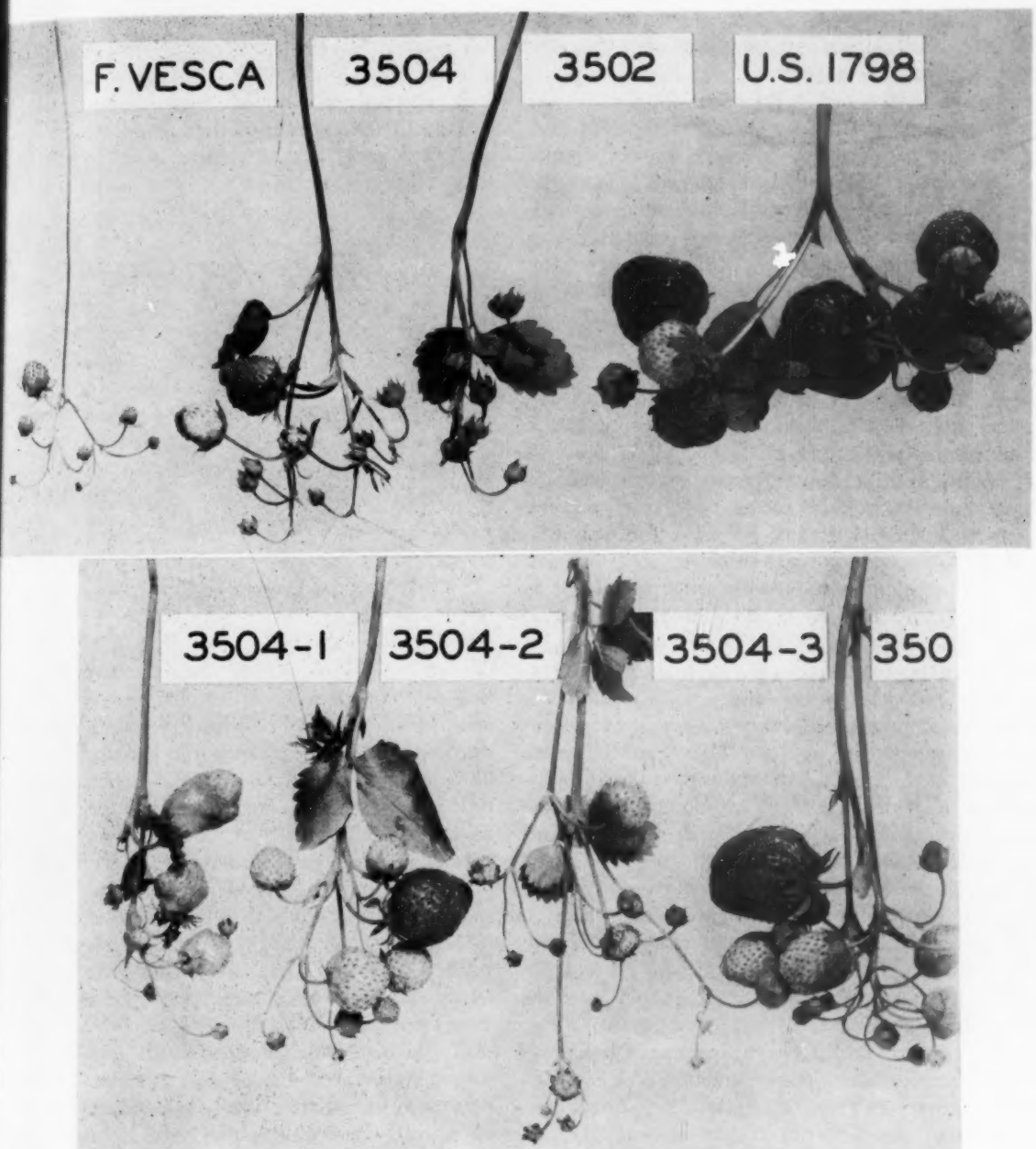
Strawberry

The geologically ancient ancestors of our modern strawberries were, of course, diploids. A basic diploid, *Fragaria vesca,* still occurs in many parts of the Northern Hemisphere in a series of forms (and under various names). It still is the most widespread native strawberry and is also now present naturalized or native in the Andes and other parts of the temperate Southern Hemisphere. It seems likely that other ancestral diploids once were present; extensive and detailed exploration may yet uncover additional living forms. Today, there occurs sporadically in central Europe another straw-





erry, *F. moschata,* a hexaploid species. It resembles *F. vesca;* its flowers and berries are somewhat larger, but its flowers are not always fertile. It is our supposition that the material called *F. moschata* originated through the union of normally reduced pollen grains with unreduced eggs, which resulted in triploid plants. These autotriploids apparently did not have the necessary fertility to survive for any great length of time as natural populations. But when such triploids double their chromosome number, hexaploids would be produced, and these would be sufficiently fertile to persist, at least locally. The sporadic nature of the present distribution of the wild *F. moschata* lends



The origin of decaploid (10-ploid) strawberries. Above to left, the diploid woodland strawberry, *Fragaria vesca;* to right, an octaploid garden strawberry. By the use of the drug colchicine, a tetraploid, *F. vesca,* was obtained. This was crossed onto the variety US-1798 at the right, and the two center hexaploid selections, US-3502 and 3504, resulted. They were nearly sterile, as shown. But when these were backcrossed to the octaploid garden strawberry, fertile seedlings (below) were obtained. They proved to be decaploids and retained the high aroma of the diploid parent.

support to this hypothesis; it appears to be a semi-fertile species that arose out of the more common and widespread diploid *F. vesca.* It is what the botanist calls a polytopic species, a species that arose at various times and in various places from a common ancestor. There is every reason to suspect that new, local, hexaploid populations of *F. moschata* may still be arising; but, because of their low fertility, they rarely would be able to form effective natural populations. Some of these sporadic local races have been selected, and named varieties are occasionally found in European gardens.

Our cultivated strawberries are octaploids and, we must suppose, also have evolved from diploid ancestral forms, possibly as autopolyploids from the same *F. vesca,* but more probably as allopolyploids from two or more allied species now extinct. There are three known octaploid species: *F. virginiana,* our Eastern meadow strawberry; *F. ovalis,* the Western field strawberry; and *F. chiloensis,* the Pacific beach strawberry—and all enter into cultivated varieties, though the Western field strawberry but a little and that only in recent years. These three species are all North American, but the beach strawberry is also found in Chile and on the tops of mountains in Hawaii. Recent reports indicate that related octaploid forms may exist on the mountains of the Island of Formosa and of southwest China. Although the diploid ancestors are probably extinct, we may yet find forms related to *F. vesca* from which by crossing, followed by doubling and redoubling of the chromosome sets, we can reconstruct species similar to each of the present octaploid wild species.

Be that as it may, with such a view of the origin of our strawberries, we can utilize much better what we have. Thus, Dermen doubled the chromosome number of *F. vesca.* This experimentally induced tetraploid was crossed with the octaploid garden strawberry; the result was a series of hexaploids. These hexaploids were nearly sterile but were backcrossed to octaploid garden strawberries, and some fertile seedlings were obtained. These fertile seedlings were found to be 7-ploids and 10-ploids; it is a pleasure to record that they preserve the desirable fragrance of the *vesca* parent. D. H. Scott is carrying on this promising line of breeding. Such success indicates the satisfaction of work with the strawberry and suggests that, actually, only pioneering work has as yet been done.

Blackberry

The blackberry furnishes a notable instance of a polyploid series in both wild species and cultivated varieties. Varieties are actually in cultivation that make up a polyploid series of 2, 3, 4, 6, 7, 8, 9, and 12 sets of chromosomes. Representatives of all of these except the 3-, 7-, and 9-ploid forms have been selected from the wild species. There are reasons to believe that many species of this native polyploid series are fairly ancient. Polyploidy has been effective in the evolution of blackberry for a long time, both in this country and in Europe, and is probably particularly effective today. Here in the Eastern states, since the forests have been cleared, there are diploid, tetraploid, and hexaploid species growing side by side—and bees are making their hundreds of millions of cross-pollinations each year. Tens or hundreds of thousands, or millions, of these crosses are between species with different chromosome numbers, and from these

BLACKBERRIES

Cultivated varieties are:
2x-4x-6x-7x-8x-9x-12x

Wild species and hybrids are:
2x-3x-4x-5x-6x-7x-8x-9x-10x-11x-12x

crosses new forms are continually arising. Furthermore, from the relatively few crosses made by scientists, Waldo has recently selected the 9-ploid Cascade variety which, in addition to being the finest-flavored of all blackberries yet known, also must be considered a new species and a new polyploid type. Recently, also, he has crossed the tetraploid Eldorado variety with an octaploid native blackberry and thus produced a whole population of hexaploids constituting what amounts to a new species of the Mammoth type. Crane and Thomas in England have produced a thornless tetraploid variety, Merton Thornless, which we can cross with hardy American tetraploids, thus opening up additional possibilities.

In the blackberry, there are still many problems in polyploidy awaiting solution. A notable one concerns the very hardy thornless blackberry, *Rubus canadensis.* We would like to use it in breeding but, thus far, have not been able to do so. Though a wide-ranging species, it is triploid everywhere in so far as is known. Bailey has surveyed the forms related to it and described some seventeen of the numerous forms of it that occur in the wild. It

may well be that a diploid, a tetraploid, and even a hexaploid may be found among them that would be extraordinarily useful to breeders in getting thornless blackberries.

Raspberry

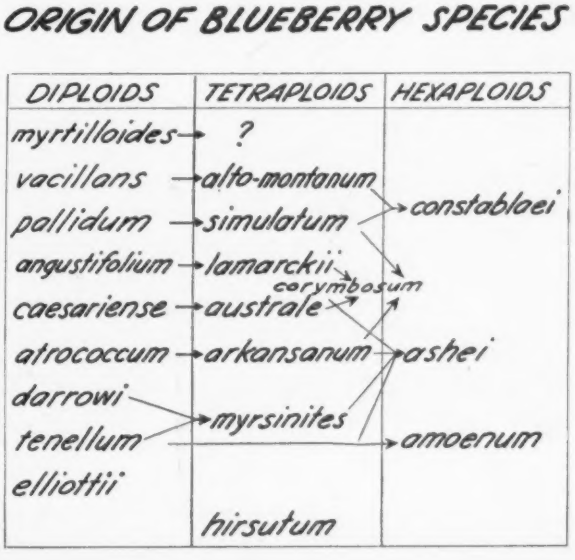
I have mentioned being forced into a position of interest in the polyploid condition of the raspberry. The discovery of the usefulness of colchicine in producing polyploids led to tests of its use on raspberry seedlings, on growing points, and on root cuttings. Treatment of very young seedlings was successful in producing polyploids, but during the war period they were lost. Nevertheless, we have in colchicine a new tool to make polyploids if we need them badly enough. There are upward of two hundred species of raspberries in Eastern Asia with new horticultural qualities that have never been utilized in breeding. If they do not prove fertile in intergroup hybrids, possibly they may if tetraploidy is induced by the use of colchicine. In each such tetraploid it is possible that the plant will function as a diploid (technically called an amphidiploid or amphiploid), and fertility will thus be restored. The opportunities are there. The small number of tetraploid red raspberries found by breeders during recent years seems to indicate that in this fruit polyploidy is just becoming established; that we are sitting on the side lines and seeing a major advance in the horticultural evolution of this fruit. Crane and Lawrence suggest that polyploidy in the red raspberry has begun only within the past eighty years.

Waldo has been successful in using one tetraploid raspberry in crossing with octaploid blackberries and has produced a whole new series of Loganberry-like fruits. Crane, in England, had already shown that ordinary reduced pollen of a diploid raspberry would not do in such a cross, that 2 sets of raspberry chromosomes were needed with 4 sets from the blackberry to make a Logan-like berry. These Logan-like berries constitute a new hexaploid species, which has been named *Rubus loganobaccus* by Bailey. It came into existence in 1880 or 1881 when an insect made the cross and Judge Logan planted the seed from which the first Loganberry came. Theoretical considerations indicate that a similar cross of a tetraploid raspberry with a 12-ploid blackberry would be fertile and might also be of great value. It would likely be an octaploid and would constitute another new species type. But most of all we need both hardier tetraploid raspberries and hardier octaploid blackberries to hybridize to produce hardier Logan types.

Blueberry

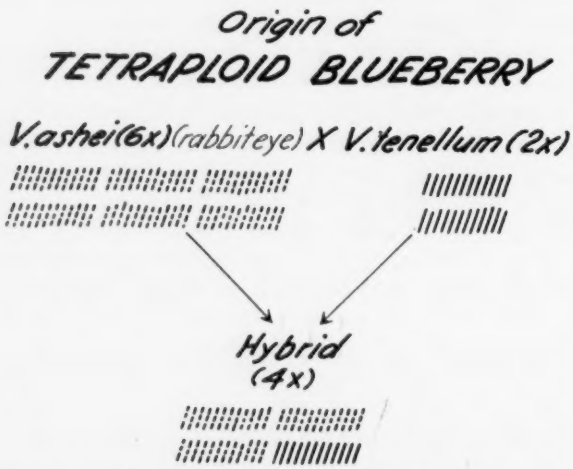
The development of the blueberry has occurred within the lifetime of most of us. But the development of the blueberry as a fruit has occurred through millions of years, and polyploidy has played a large part in the process. The cluster-fruited blueberries in which scientists are interested are mostly eastern North American. There are about seven or eight diploid species, seven or eight tetraploid species, and three hexaploid species. It is particularly interesting to find both diploids and tetraploids of several of the same types of blueberries still growing in the same areas. Evolution in the wild is very active in this fruit today, just as it is in the blackberry. Several blueberry species are important in that their fruit is harvested in the wild, but those that have been most important in the origin of cultivated varieties are the tetraploid highbush, mostly of the Atlantic Coastal region, the tetraploid lowbush of the Northeast, and the hexaploid rabbiteye of southern Georgia and northern Florida.

In the Southern states one ancient species is the diploid *Vaccinium tenellum,* a lowbush drought- and heat-resistant form. In part from this species, and also from a series of common ancestral species, the drought- and heat-resistant hexaploids *V. amoenum* and *V. ashei* (the rabbiteye) have evolved—the rabbiteye with the most vigorous and productive bush of all blueberries. The hexaploid *V. amoenum* is widely distributed in the Southern states and may have evolved before the Ice Age from the diploid *V. tenellum.* The hexaploid rabbiteye appears to be younger, seemingly having developed, in part, only within the past 15,000–20,000 years, much of this development having



taken place within the past century. The majority of the plants of it found in western Florida grow in what were rice fields, which the rabbiteye blueberry invaded when the plantations were abandoned during the War Between the States. We have found no tetraploid of the rabbiteye group. We therefore crossed the diploid with the hexaploid and this past year have obtained a tetraploid; this has already been used to cross with the highbush to obtain hybrids with the earliness of the highbush and the plant qualities of the rabbiteye. Though we have experimentally produced a new and, we hope, very useful species just this past year, it is hardly possible that nature has not already evolved this or closely similar species, for the hexaploid and diploid species are growing together in the South. As occurs so often, we have not sufficiently surveyed what nature has done for us and, in this case, have found it easier to make what we want.

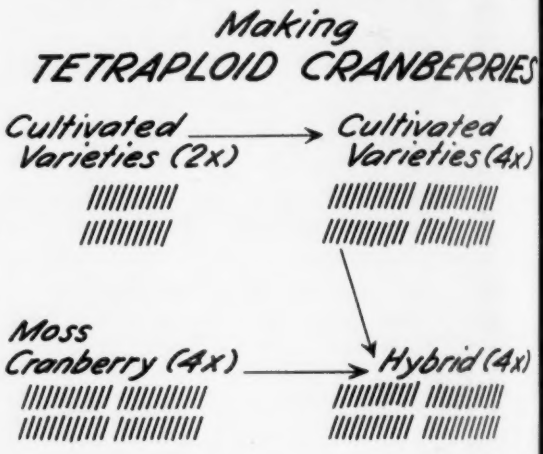
Stanley Johnston surveyed the lowbush blueberry of Michigan to select the best individuals for crossing with the highbush. Among the selections was Michigan lowbush #1. It seems to be, however, what we had been searching for—a tetraploid



hybrid involving genes both of the very hardy light-blue Canadian blueberry, *V. myrtilloides,* and of the lowbush species. It is probable that an unreduced pollen grain of the diploid Canadian blueberry fertilized a normal egg of the tetraploid lowbush to produce the Michigan #1, or its ancestor. At any rate, in this selection, or in others yet to be discovered, we should find some of the desirable qualities of this Canadian blueberry species that we wish to utilize in breeding.

Cranberry

There are at present three recognized species of cranberry in the world; two of these are diploid, and the third is tetraploid. In addition, scattered hexaploid plants have been found, but these appear to be self-sterile and so produce no fruit in the wild. Little is known about them, for they are not yet in cultivation. As in the case of the blueberry, the diploid and tetraploid cranberries do not cross. The large acreages of cultivated varieties are now composed entirely of diploids, derived from the southernmost species. A more northerly and hardier species is tetraploid. To cross the commercial diploid varieties with the wild tetraploid, Dermen and Bain have proceeded as follows: They used colchicine to produce tetraploids of a majority of



the cultivated varieties; these were then crossed with individuals of the more northerly wild tetraploid. The induced tetraploids of the cultivated varieties have been crossed and selfed, and these are now in the field for fruiting tests. The results are most promising. As yet there has been no opportunity to cross the extremely hardy high-arctic diploid species with the Southern, commercial diploid varieties. When this is done, the best of the resultant hybrids could then be changed into tetraploids by the use of colchicine; following this, the material then could be hybridized with the best of the new tetraploid races now undergoing field tests. The final selections would probably be a series of new super-hardy cranberries.

Grapes

With the experience with small fruits, briefly described, our interest has been greatly aroused by the reports of Olmo on his observations and studies on tetraploid grapes and by the samples of tetraploids of both Portland and Fredonia varieties seen during the past three years. The clusters of the tetraploid forms of these two varieties were fully developed and the berries almost exactly twice the weight of the corresponding diploids. The size

and character of tetraploids of varieties, and Olmo's report on the very large size of berries of his seedling tetraploids, indicate that breeding of grapes on the tetraploid level may be of immediate value in obtaining very large-fruited commercial varieties.

Apples

Commercial apple and pear varieties are either diploid or triploid. Triploid apples and pears are exceptional among fruits in being sufficiently productive to become important varieties, though they develop few seeds, require cross-pollination, and are poor pollinators. The triploid apple varieties such as Baldwin, Stayman Winesap, Arkansas, and Rhode Island Greening have all been chance seedlings. They develop into large, excellent trees, and we would like to produce triploids at will in breeding.

Nilsson of Sweden has reported obtaining tetraploid seedlings from triploid varieties and has already used the tetraploids extensively in breeding to obtain thousands of triploids and some additional tetraploids.

Giant-fruited apple sports have been collected by V. R. Gardner and R. Gibson at the Graham Station, Michigan, and W. H. Thies has propagated a giant McIntosh at the Massachusetts Station. These and other facts, with the small-fruit experience as a background, suggested that giantism in the apple is due to tetraploidy. Studies by Dermen and by Einset have shown this to be the case. It has been demonstrated that many of the trees producing the giants are chimeral diploid-tetraploid mixtures having a tetraploid cortex, stele, and pith of the trunk and branches inside a diploid epidermis, and a diploid outer cortex. The origin of the reproductive tissues is from the diploid outer cortex; however, Dermen is now attempting to make full tetraploids of all the giant varieties through adventitious bud development, so that breeding may be done on the tetraploid level with our best varieties, or so that tetraploids of known varieties may be crossed with diploids to make triploids.

Pears, Peaches, and Cherries

Most American pears are diploids, but many triploid varieties are grown in Europe. Two giant sports, both tetraploids, have recently been reported—one of Bartlett in the United States and one of Fertility in England. The variety Fertility is self-sterile on the diploid level but self-fertile as a tetraploid. By the use of colchicine, Dermen has obtained tetraploids of the Michigan #504 pear and some others, has used the tetraploid #504 in a limited amount of crossing with diploids, and has obtained a dozen triploid plants.

In the peach, until recently, only diploid varieties have been known. However, tetraploids of the Golden Jubilee, Halehaven, and Elberta varieties, and an unnamed selection, have now been obtained by the colchicine technique; when they blossom, breeding can begin on the tetraploid level. One chance triploid peach has proved to be completely sterile, although it appears to have some good pollen.

The sweet cherries are diploid, the sour cherries tetraploid, and the Dukes, supposed hybrids between the sweet and sour, also are tetraploids. How the tetraploid Dukes originated from tetraploid × diploid parents is not yet fully known. It is known, however, that unreduced pollen is produced at times.

PLOIDY OF FRUITS

Apple & Pear	2x	(most varieties)
	3x	(some varieties)
	4x	(rare)
Peach & Apricot	2x	(all)
Plum	2x	(many varieties)
	4x	(few)
	6x	(many varieties)
Cherry	2x	(nearly all sweets)
	4x	(all Dukes and sours)
Grape	2x	(nearly all)
	4x	(rare)

In review, then, it should be noted that ways have been found to combine the fragrance of the diploid *Fragaria vesca* with the other good qualities of the cultivated octaploid strawberry. A needed type of tetraploid blueberry has been made to order. The origin of the Logan and Mammoth blackberries has been determined and many new Logan- and Mammoth-type berries originated. A tetraploid thornless blackberry is now available for producing hardy new thornless ones. The new 9-ploid Cascade blackberry, the finest in flavor of all known varieties, is now being grown in the Pacific Northwest. And the hardiness of a more Northern tetraploid cranberry has been combined with the horticulturally better diploid varieties on the tetraploid level. There is still a wealth of wild forms not yet sur-

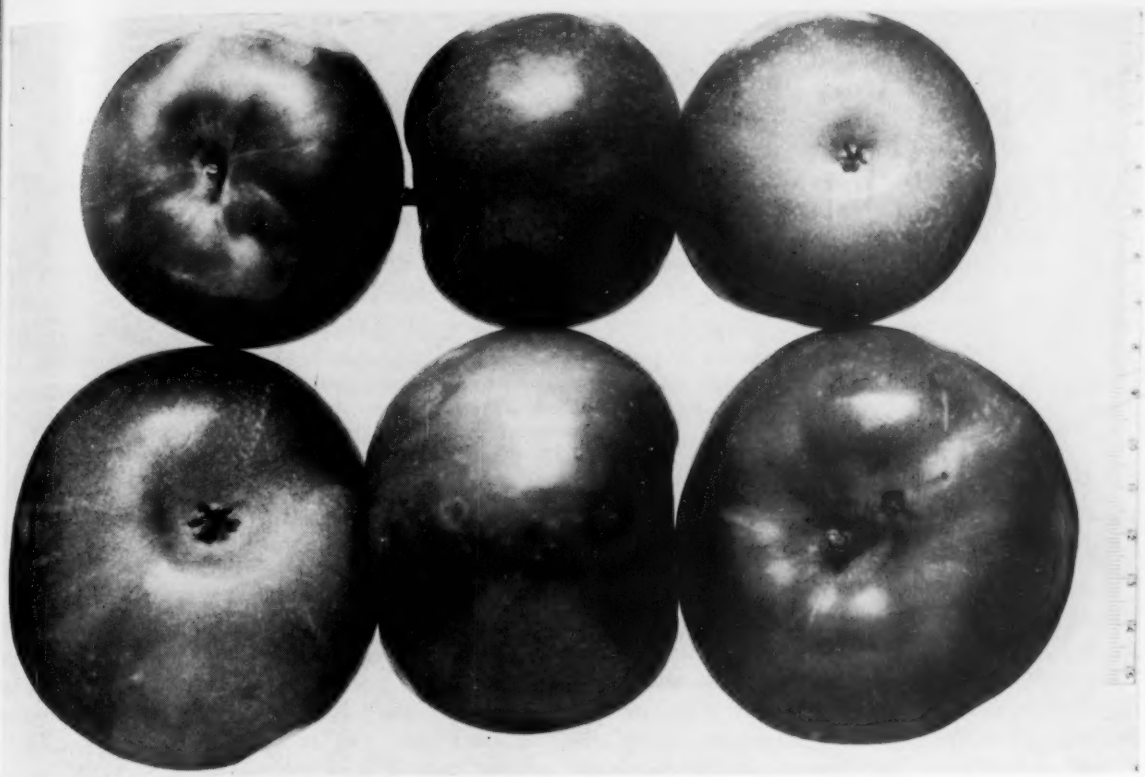
veyed that will furnish the superior genes for the production of much finer berries.

In this summary of the polyploid condition of fruits it becomes evident that, in many of them, polyploids are much superior to diploids and have already replaced the diploids wholly or in part, occasionally in nature and often in cultivation. The advantages of cultivated polyploids are evident. Octaploid strawberries are many times larger and more productive than the diploids. Tetraploid and hexaploid blueberries are larger than the diploids. Most of our cultivated blackberry varieties are tetraploids, selected from the wild for their size, productiveness, and flavor. Fruit of tetraploid grapes, apples, and pears are much larger than the diploid forms. Self-sterility of the diploid in some instances is replaced by self-fertility in the polyploid. The vigor of many polyploid plants is an expression of better adaptation to their present environment as, for example, greater drought- and heat-resistance, better utilization of soil fertility, or deeper root system of the octaploid strawberries, as compared with the diploid forms. The tree of triploid apples is generally more vigorous and the branching stronger than in the diploids.

There is, at present, some objection on the part of the consuming public to these larger fruits produced on biologically more efficient plants. The greatest complaint is that they lack the flavor of the smaller, old-fashioned sorts, most of which we now know were diploids. This is not the fault of the polyploid condition; it goes back to the ancestral material used. For example: Let us keep in mind that the genes of the diploid *F. vesca,* probably the most highly flavored of the wild species, did not directly enter into the make-up of the octaploid commercial strawberries; this has now been accomplished by breeders. It is equally true that the first of the polyploid blueberries introduced into cultivation did not carry the fine flavor of some of the wild sorts. In the early years of blueberry breeding it was not possible to produce the proper combinations. With the newer techniques the necessary polyploids have been produced with a level of flavor satisfactory to the taste of the most discriminating consumer. The new blueberry varieties now being introduced are evidence of what can be done in controlling this important fruit character. The more we know of how polyploidy operates to produce new species and varieties, the better we shall be able to use polyploidy as a means of improving our fruits.

Polyploidy induction is another tool to be used by breeders along with hybridization to obtain desired improvements. The effects most often obtained

To the left, a normal McIntosh apple tree; to the right, a tetraploid McIntosh. Note the erect growth of the normal and the generally horizontal branching habit of the tetraploid.



In the apple the tetraploid produces fruit approximately twice the size of the normal diploid. In the comparison shown here, the three tetraploid McIntosh weighed 788 grams, and the three normal diploids 427 grams.

through polyploidy are broader, thicker leaves, larger flowers, and larger fruits. Other desirable qualities may also be obtained, but thus far cannot be predicted. Polyploidy in nature has been associated with hybridization, and hybridization followed by doubling the chromosome number has given us most of our fruits.

It has been suggested that plants with basic chromosome numbers of 10 and higher may be of polyploid derivation. If this be true, only the peach, apricot, sweet cherry, raspberry, and a few blackberry varieties are not of polyploid origin. The apple, pear, and quince are now considered polyploids derived from crosses of 8- with 9-chromosome forms, with subsequent doubling of the chromosome number. The apple, pear, and grape may all be thought of as ancient polyploids. It may not be an impossibility to construct other new fruits from intergeneric crosses rather than just interspecific ones.

We have learned just a little of the effect of polyploidy on our cultivated fruits, but that little is good and indicates the possibilities ahead. Also, we have learned more of the ways in which diploids can be transformed into tetraploids and of how even higher polyploids can be produced. Though progress may not seem to be so rapid with these slower-developing woody plants as with annual crops, very significant and important developments are being made.

In this very brief review of polyploidy in relation to fruit improvement, the viewpoint has been that of a builder surveying the possibilities in his material and the usefulness of his tools. The pile of material is enormous. There seem to be no unsurmountable barriers to the use of any of the material. The tools are now available to fashion this material so that the builder can create exactly what he wishes. The only limits are his blueprint, his time, and his industry.

Soap Solutions: Salt, Alcohol, Micelles, Rubber

WILLIAM DRAPER HARKINS

Dr. Harkins (Ph.D., Stanford, 1908) has been Andrew MacLeish distinguished service professor of chemistry at the University of Chicago since 1935. The investigation he discusses was carried out under the sponsorship of the Reconstruction Finance Corporation, Office of Rubber Reserve, in connection with the government's synthetic rubber program.

Importance of Soap Solutions

THE winning of World War II was contingent on the development of an industry capable of supplying large quantities of synthetic rubber. This is universally realized, but it is not so well known that without soap and its micelles the supply of rubber would have been altogether insufficient. Since very early times soap, or the lack of it, has played a most important role in the history of mankind. Soap and water together have done much to improve the health of human beings and are indispensable from the aesthetic point of view; indeed, the extent to which a country has advanced in material civilization is measured by its consumption of soap.

In the solid state certain soaps have important uses, but the most numerous and significant applications arise from solutions of soaps, principally in water. Merely to list them would require a paper longer than this one. Probably no one human being knows all the ways in which soap is applied.

Soap is commonly defined as the salt of a long-chain fatty acid. However, as used here, the term includes all colloidal electrolytes which exhibit the general properties that distinguish the "true" soaps. For example, sodium lauryl sulfate is widely used for cleansing, but it is not the salt of a fatty acid. It has the advantage over a fatty acid soap in that in general it does not form a precipitate in hard water, but it has perhaps a disadvantage in that it is possibly less soothing to the skin. Such materials are commonly designated as detergents, which is unfortunate, since cleansing action is only one of the many types of behavior they exhibit. For example, Harkins has been engaged with Corrin, Stearns, Mattoon, Mittelmann, Herzfeld, Oppenheimer, and others in an extensive investigation, with many types of techniques, of the properties of soap solutions, but with no interest in their cleansing action. Of all the salts known, soaps exhibit the most complicated and the most interesting behavior in solutions.

Behavior of Soaps in Nonmicellar Solutions

In sufficiently dilute aqueous solutions soaps are ionized into positive and negative ions, just as sodium chloride is ionized. The difference is that in the former one of the ions is large, and is in many cases a long hydrocarbon chain. Thus the laurate, myristate, and palmitate have chains 12, 14, and 16 carbon atoms long. Such large ions are highly adsorbed at the interface between water as one phase and oil, graphite, or any nonpolar phase as the other, where they form a monomolecular film. The molecules in this film are oriented with their nonpolar ends toward the nonpolar phase and the polar charged ends toward the water.

It is this film which gives a charge to oil drops in an oil-in-water emulsion and a degree of stability to the emulsion which is dependent on the concentration of the soap. Thus an emulsion of a paraffin oil in water, after very vigorous agitation, may remain stable for several years in 0.1 molar sodium oleate solutions, but only for a very few minutes in a 0.001 molar solution.

Experiments carried out in several laboratories on the washing of cotton cloth in solutions of colloidal electrolytes such as sodium lauryl sulfate indicate that the rate of cleansing is nearly proportional to the concentration of the ions in the solution, and it is undoubtedly the large, nonmetal ions that give most of this cleansing action. For soaps of the fatty acids, however, Tomlinson finds that the cleansing power for cotton decreases with increase in concentration in the region in which hydrolysis begins, and later increases rapidly up to the critical micelle concentration (cmc). The soap helps in the wetting of the cloth by the solution, and covers not

only the fibers of the cotton, but also the grease and solid particles, or "dirt," with a monolayer of these ions. The oils are emulsified, and the solid particles are suspended, by a process entirely similar to emulsification. For the cleansing of the skin micellar solutions seem to be more efficient.

Since the large ions of the soap give their charge to the emulsion droplets and to the solid suspended particles, the charge on these ions may be very important as related, in the case of different types of cloth, to the charge on the cloth. The large ions of all true soaps are negatively charged and thus impart a negative charge to the emulsion droplets or solid particles. An example of a soap in which the large ion is positively charged is given by lauryl ammonium chloride and similar soaps.

As a preparation for dyeing, cotton, woolen, and various other types of cloth are treated with various types of soap in solution. Here the sign of the charge on the surface of the cloth and also that on the large ion of the soap are important, since the purpose is to coat the fibers of the cloth with a monolayer of the large ions. Also, both the structure and size of the large ion have a considerable effect upon the results achieved.

Micelles: Their Behavior

It is the belief of the writer that none of the models of the micelle proposed by others, and there are many of them, is in agreement with all the results brought out by the numerous investigations carried out to determine the structure of groups of ions, designated as micelles, which exist in aqueous soap solutions.

Soap solutions that are not too dilute exhibit extremely remarkable properties, which show so many facets of behavior that few of them can be even mentioned here. Taken together, these prove definitely that much of the soap, often nearly all of it, exists in the form of aggregates of molecules, which are not tied together chemically. These aggregates often contain 20–200 molecules, although they may be somewhat smaller or larger. It has been shown in this laboratory that in solutions of any pure soap *the X-ray value which is assumed to be related to the diameter of the micelle is independent of the concentration of the soap.*

Many attempts have been made to explain one puzzle exhibited in the behavior of micellar solutions: their conductivity is high, which seemed to indicate many charged particles, and their osmotic pressure is low, which seemed to indicate few particles. To explain this anomaly McBain attributed it to the properties of the micelle, and gave the explanation that there are two types of micelles: one a small, spherical, hydrated, and highly ionized aggregate, and the other a large, lamellar, and weakly conducting particle. Hartley, much later, considered that only the spherical form exists.

The extensive X-ray work on soap solutions carried out by a number of German investigators in the late 1930s seemed to give definite evidence in favor of the large lamellar micelle. However, both a new X-ray band found in this laboratory and the amount of oil solubilized by the micelle do not agree with the lamellar model. Thus such a micelle does not exist in soap solutions. The evidence for this conclusion has been published in a number of papers from this laboratory, but this is too extensive to consider in detail here.

Critical micelle concentration. One remarkable characteristic of soap solutions is that nearly all their properties undergo a marked change in a concentration range which, with very long-chain ions, is extremely narrow. The specific conductivity is a linear function of the concentration both below and above this cmc except when hydrolysis is involved, but increases much less rapidly when micelles are present (Tartar). The intersection of the two straight lines may be considered as the cmc. The equivalent conductivity in the absence of micelles exhibits a very slight, almost linear, fall against the square root of the concentration, with a slope about that predicted by the Onsager equation. As soon as micelles appear, the equivalent conductivity begins to fall with surprising abruptness.

The density of the solutions gives the same type of variation as the specific conductance (Bury). Its variation with concentration is represented by two straight lines whose intersection may be considered as the cmc. This linear density relationship is not exhibited by sulfonates (Tartar).

Only one of the many other methods of determining the cmc is considered here. Certain dyes in extremely dilute concentration exhibit changes in color (Fig. 1) or in fluorescence at the cmc (Corrin and Harkins). If the soap is anionic (e.g., sodium myristate) the dye must be cationic (e.g., pinacyanol chloride). This dye gives one color when micelles are present, another when they are absent. Eosin, an anionic dye, exhibits with cationic soaps a high fluorescence and an orange color when micelles are present, and a red color, with quenching of fluorescence, just below the cmc.

In looking over the critical concentrations obtained by Corrin and later by Herzfeld with this method, a remarkable relation was found: *the critical concentration is doubled for each reduction by unity in the number of carbon atoms in the hydrocarbon chain.* This is equivalent qualitatively

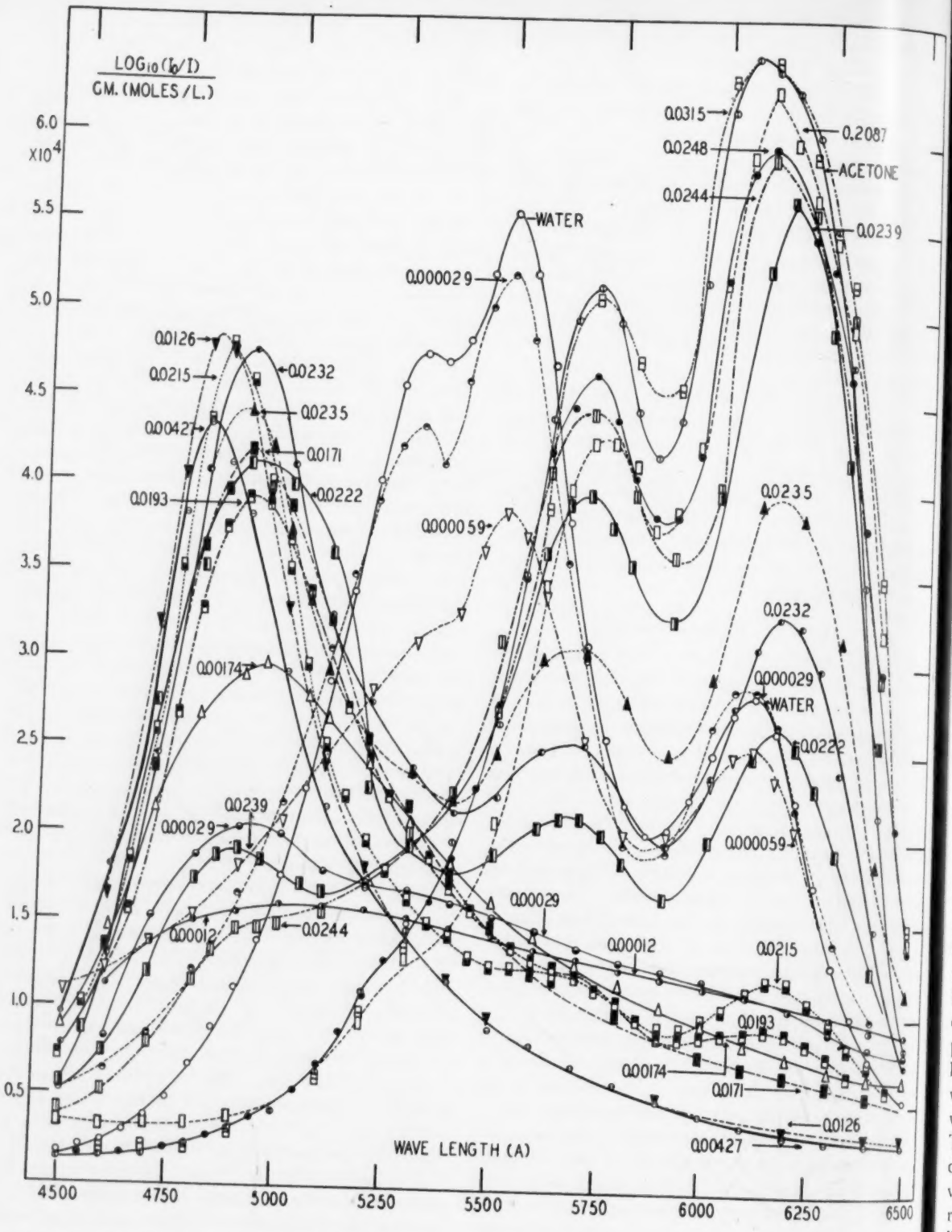


FIG. 1. The extremely great effect of changes of soap concentration upon the absorption spectrum of a dye. Maximum concentration of soap: 0.0315 moles per liter.

to a logarithmic relation which was known earlier. Thus potassium or sodium hexadecanoate (16 C) has a cmc of 0.0015 molar. To obtain the value for the 10 C atom soap this is multiplied by 2^6, which gives 0.096. This is as accurate as the experimental value. For other "detergents" the factor, for cases in which it has been determined, is the same, but the values are different—e.g., 0.0065 for sodium dodecyl sulfate. For the fatty acid soaps the factor of 2 may be expressed as follows: $lnC_k = 4.811–0.714\mu$, in which C_k is the critical concentration and μ the number of carbon atoms, or the energy equivalent of one $-CH_2$ group is 0.714, or $3kT = 3 \times 0.714kT–2.14\ kT$. Debye has shown that this gives 1,280 cal per mole of $-CH_2$. The interfacial energy values determined in this laboratory show that this is a very reasonable value for the decrease in free energy when a hydrocarbon molecule is removed from water into a hydrocarbon.

In the early work of this laboratory (1917–22) a very important simple fact concerning molecular interaction was discovered. This has been recognized since by almost no one but Hartley. The attraction in terms of free energy is essentially the same between hydrocarbons and water as in hydrocarbons themselves. The molecular attraction in water is very much higher, however. The central portion of a micelle resembles a liquid hydrocarbon in that the X-ray diffraction pattern obtained from a micellar soap solution displays one maximum which is similar in both position and half-width to that observed in diffraction from liquid hydrocarbons. Thus the 1,280 cal per mole of $-CH_2$ groups is not due, as certain workers have supposed, to any greater attraction of hydrocarbon for hydrocarbon, but to the greater attraction of water for water, since when a hydrocarbon chain moves out of the water it allows the water molecules to come together. The first application to micelles of this relation as found by Harkins was made by Hartley, but unfortunately other investigators have not recognized that this is what is involved. For bulk phases the free energy increase per sq cm when a long-chain hydrocarbon is separated from either water or from a hydrocarbon is *ca.* 45 ergs, but water from water involves an increase of 144 ergs. Thus when water comes in contact with water the decrease of free energy is 144 ergs. Although the values on a molecular scale are not the same, these relations show that the cause of micelle formation lies in the free energy decrease in the coming together of water molecules when hydrocarbon groups move out. The change of free energy related to the attraction between water and hydrocarbon, on the one hand, and hydrocarbon and hydrocarbon, on the other, is close to zero, so it does not materially affect the results.

If the hydrocarbon alone were involved it would separate as a bulk phase. That micelles are not usually extremely large is due to the repulsion of the ionic heads of the ions. This causes an increase in the free energy of association. When the free energy decrease related to the hydrocarbon chains and this increase due to ionic repulsion are exactly balanced, the size of the micelle is determined. Thus under given conditions the micelle has a definite size.

Structure of the micelle. All the structures proposed for the micelle since 1917 have one aspect in common: they are all based upon the theory of molecular orientation (Fig. 2) in interfaces as proposed (1917) by Harkins and by Langmuir, particularly upon the energy relations discovered by the former. In one of their papers Harkins, Davies, and Clark showed that soap forms a monolayer on the surface of water with its $-\overset{\overset{O}{\parallel}}{C}-O^-\ Na$ "head" groups turned toward the water and the long hydrocarbon chains toward the vapor. The surface tension was shown to decrease extremely rapidly with concentration until the cmc is reached, but beyond this is almost constant. The area per molecule in this monolayer is, on account of the repulsion of the ionic heads, much larger than that for the corresponding acid (*ca.* 21 sq A).

Let us consider the case of potassium myristate with a chain length of 14 carbon atoms and a molecule 20 A long. A micelle of this soap contains at the critical concentration about 70 molecules. Since the criterion for the formation of micelles is as complete as possible removal of the hydrocarbon chains from the water, we may think as a first approximation that the form of the micelle is a rough sphere of radius 20 A. This is in line with the views of Hartley; from diffusion experiments he and Runnicles calculated that for cetyl pyridinium chloride (16 C) 70 paraffin chains are present per micelle. The area occupied per long-chain ion on the surface of graphite at an equilibrium concentration equal to that of the critical concentration is approximately 55 A^2 for potassium myristate. If this is of the same order of magnitude as the area occupied per molecule in the micelle, the number of molecules per micelle calculated on the basis of a sphere 20 A in radius is reasonable. There are no objections on this model with respect to packing of the hydrocarbon chains within the sphere.

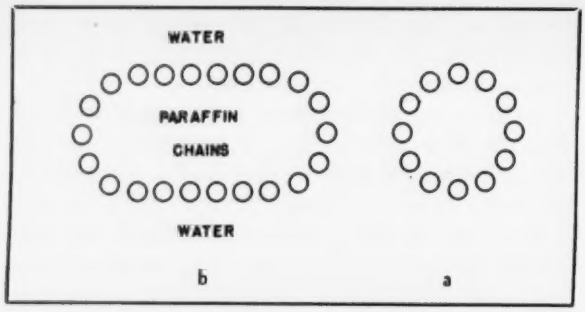


FIG. 2. (*a*) Cross section of highly idealized spherical micelle. (*b*) Longitudinal cross section of larger micelle. For either, the interior consists of paraffin chains of the soap, and, on account of the relatively large cross section of the soap molecule as compared with the size of the micelle, the ionic groups represented by the circles exhibit a very rough instead of a smooth surface.

If the micelle size is greater than approximately 100 molecules, the spherical model no longer is appropriate. There is some evidence to indicate that sizes above this limit occur when salt is added to the soap solutions. In this instance the proper model is probably a cylinder with a radius equal to the length of the soap molecule and of suitable length. The ends, of course, must be considered as rounded off (Fig. 2).

It should be pointed out that any form proposed for the micelle is true only statistically; i.e., that with dimensions of the same order of magnitude as atomic dimensions comparatively large-scale relative irregularities will exist.

Two recent discoveries in this laboratory have an important bearing on the problem of micellar structure. An X-ray band has been found which may be interpreted as a measure of the micelle diameter (Fig. 3). Diameters calculated from the position of this band are independent of soap concentration.

Mean calculated values for soaps of 8, 10, 12, and 14 carbon atoms are 30, 32, 35, and 40 A, respectively, which are in agreement, within experimental error, with twice the length of the respective soap molecules. The diameter of the sphere or cylinder as calculated from the X-ray scattering thus appears independent of soap concentration and approximately equal to twice the length of the soap molecule.

The *lowest* area exhibited by soap films adsorbed on graphite (hydrophobic) was 50 sq A for sodium dodecyl sulfate, and 35 sq A for potassium myristate (Fig. 4). The area of a hydrocarbon chain is about 18 sq A. Thus the outer area of the micelle is much larger than the sum of the areas of its hydrocarbon chains. Although a head itself has a somewhat larger area than a chain, the still larger area produced by the repulsion of the ionic heads allows some water molecules to penetrate between them.

Structural relations. Obviously this larger area of the outside of the micelle, due to the heads, allows more space inside for the packing of the chains. The short spacing X-ray band which gives *ca.* 27 sq A as the area per chain has an appearance almost exactly the same in diffuseness as the liquid water band. Thus in this respect the hydrocarbon interior of the micelle acts as a liquid.

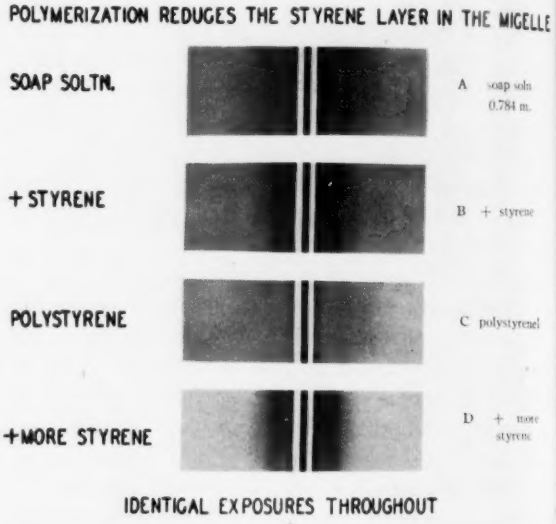


FIG. 3. X-ray band. (*A*: Bands on each side of center give the distance between micelles in an 0.784 molar solution of potassium laurate and myristate 0.392 molar for each soap. A second band gives diameter of the micelle as 35.3 A. *B*: On saturation with styrene at 25°, the diameter of micelle is increased by 11.50 to 46.8 A, exhibited by a decrease in distance of the bands from the central line. *C*: The styrene has been polymerized to polystyrene, the bands are weakened in intensity, and the diameter of the micelles that remain is the same as that in *A*, where no styrene is present. *D*: More styrene has been added, and the diameter of the micelle is approximately that in *B* after the first addition of styrene.)

It would seem, then, that the *pseudo* interfacial tension between the micelle and the water would draw the micelle into a sphere. However, this is not a necessity, since the energy relations may give the micelle a number of molecules such that they do not fit into a sphere. Another factor is that in the extremely small space the hydrocarbon chains have a cross section and area such that, even as flexible as they are, they may give an energy of deformation such that this cannot fit into a sphere. It should be realized also that the interfacial tension between soap and water is extremely small. In fact, it is not unlikely that these factors are such that even the *cross section* of a cylindrical micelle perpendicular to its length may depart somewhat from the form of a circle.

Salts and the Principle of Ionic Strength (Fig. 5)

That salts have an extremely large effect in lowering the critical micelle concentration has been shown by extensive experiments of Corrin and Harkins, in which their dye method was used. These show that with anionic (negative) micelles the magnitude of the lowering is the same for singly, doubly, and triply charged negative ions (Cl^-, $SO_4^=$, etc.) Thus the principle of ionic strength and the Debye-Hückel theory fail to hold. This is because the (small) ions of the same charge as the micelle ion are repelled to a distance far greater than that for a univalent electrolyte (100 A for 0.001 molar and 10 A for 0.1 molar), *so the effect on the micelle is, for example, the same if a charge of 3 is on one or distributed on 3 ions.* Obviously the same is true for cationic soaps with respect to positive ions of the salt.

The great lowering of the critical micelle concentration by salts suggests that they may increase the size of the micelle. Thus an increase in length of the chain of a soap from 7 to 14 $-CH_2$ groups doubles the decrease in free energy of the hydrocarbon term in micelle formation without greatly affecting the repulsive term, so there is a large increase in the number of soap ions present.

The addition of a salt screens the action of the ions and thus reduces the repulsive work which limits the size of the micelle. The critical concentration of tetradecyl trimethyl bromide is 0.00341 molar and, with 0.0130 molar KBr present, is 0.00176, a lowering of *ca.* 50 per cent. By light scattering Debye and Anacker find that the corresponding increase in size is from 75 to 90 soap ions.

Commercial "detergents" contain very high percentages of salts, chlorides, sulfates, phosphates, etc. (often 50 per cent), so the cmc should be greatly lowered, micellar size greatly increased, and micelles made to appear at chain lengths too short for micelles to appear in the solution of the soap alone. Soap and detergent manufacturers seem to have little interest in the theoretical aspects of their work, or they would have realized this.

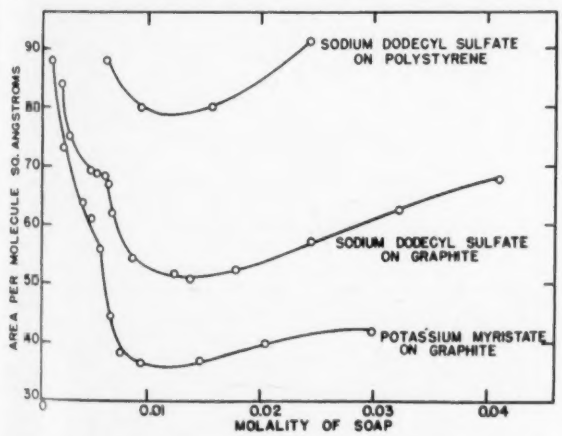


FIG. 4. Remarkable effects of soap concentration on the area per molecule of soap adsorbed on graphite or on polystyrene.

Penetration of Micelles by Alcohols

The effect of alcohols on micelles is extremely interesting. Several years ago the writer, on the basis of a knowledge of the phenomenon of the penetration of soap molecules into insoluble monolayers of alcohols on water, predicted that alcohol molecules would penetrate into the monolayer of soap which, when rolled into a sphere, forms the micelle. It was found in the work of Corrin and Herzfeld that, as predicted, long-chain alcohols greatly lower the critical micelle concentration, and that when both alcohols and salts are present they act independently. That the alcohol molecules lie between the soap molecules, with their polar groups toward the water, is shown by X-ray measurements, which indicate that they do not increase the diameter of the micelle.

In fact, the work of Harkins and Mittelmann reveals an important fact: with a long-chain soap —e.g., 12–16 C atoms—the addition of an alcohol of about half this chain length may shrink the diameter of the micelle by as much as 15 per cent, or by about 7 A. This is probably due to the shortness of the alcohol chains, which would allow a vacancy in the middle of the micelle, thus allowing some of the soap chains to fold into this vacant space. Figure 6 exhibits the extremely great lowering of the soap concentration (cmc) at which micelles form by very small amounts of long-chain alcohols.

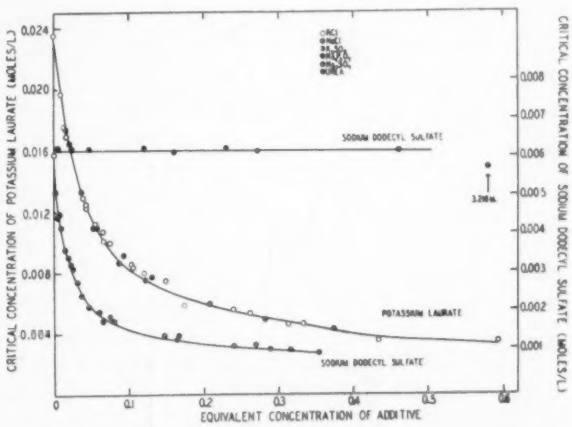


FIG. 5. Effect of salts upon the critical concentration at which soap micelles form. The fact that the values of all salts lie on a single curve shows that in solutions of colloidal electrolytes the Debye-Hückel principle is not valid.

Solubilization

Since the middle of a micelle resembles a liquid paraffin, it dissolves hydrocarbons, thus increasing the diameter of the micelle (by as much as 12 A with certain oils). This is a well-known phenomenon, but the magnitude of the increase, as measured by others, is incorrect since they used the wrong X-ray band.

The results obtained here differ from those of McBain in certain particulars: (1) It is found that the increase in the amount of oil solubilized is almost linear with soap concentration, but that (2) the amount solubilized (S) increases faster at the higher concentrations, i.e., $\frac{\delta s}{\delta c} t$ increases slightly with c, the soap concentration. This suggests, but does not prove, that, although X-rays show that their diameter remains constant, the micellar size may increase slightly with c.

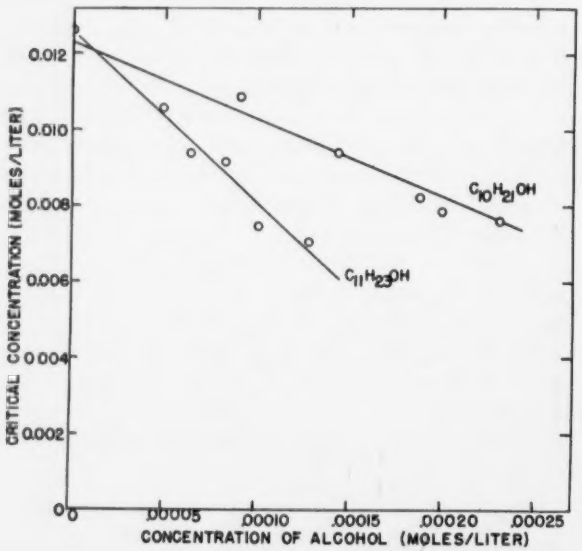


FIG. 6. Showing the extremely great effect of long-chain alcohols upon the critical micelle concentration of a soap (lauryl amine hydrochloride).

Micelles and Rubber (Fig. 7)

Essentially all the rubber manufactured in the United States has been produced as a result of the action of micelles. Most of the rubber has been formed from a mixture of two monomers: 75 per cent butadiene and 25 per cent styrene, both chemically unsaturated oils and present as liquid droplets emulsified by soap ions. These ions form an adsorbed monolayer around each emulsion droplet, with about 10 million long-chain ions around each emulsion droplet at a diameter of 10,000 A. This is about the diameter at the peak

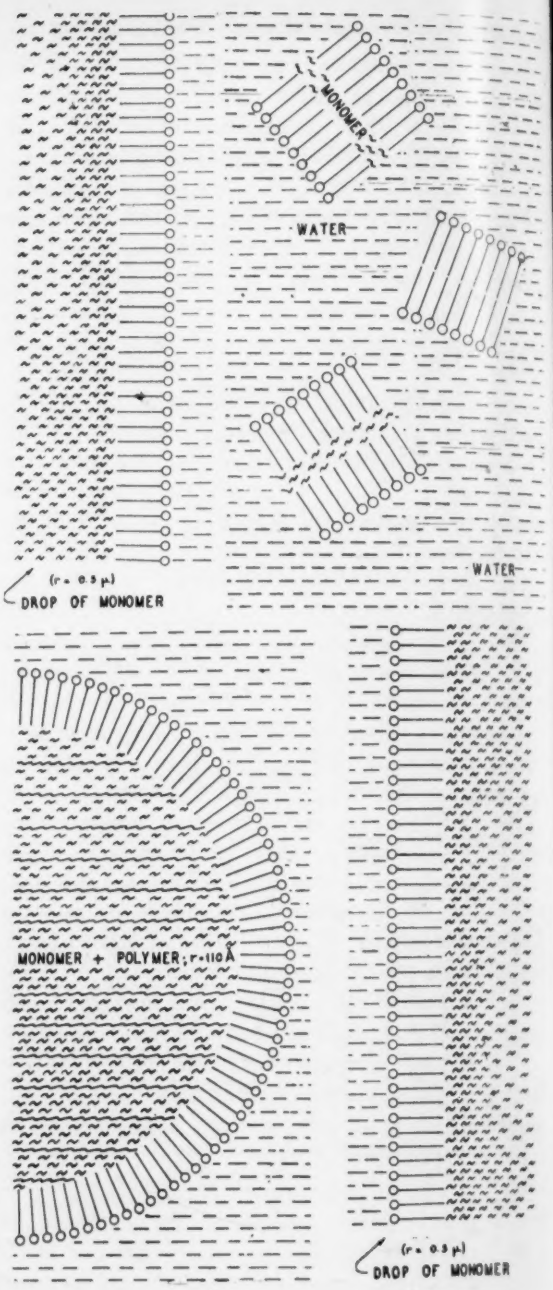
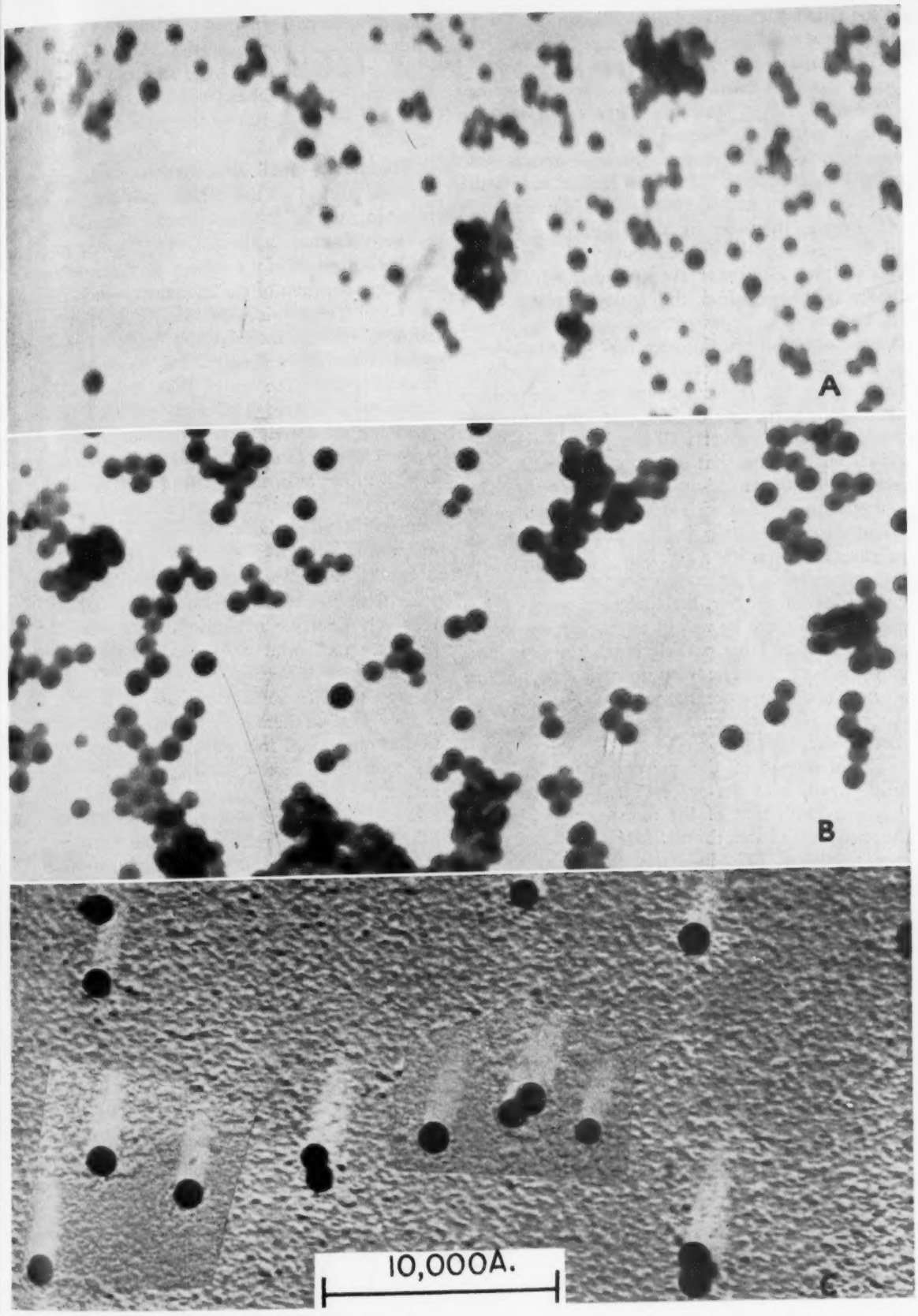


FIG. 7. Highly idealized representation of the diffusion from a drop (diameter *ca.* 10,000 A) of monomer (butadiene and styrene) into (*a*) above: micelles; (*b*) below: into a polymer-monomer droplet, diameter *ca.* 1,000 A. All the monolayers of soap, and also the micelles, exhibit a more irregular structure than that shown. (For the shapes of the micelle, see Figure 2.)

FIG. 8. Electron microscope photographs by Eckert. Black circles represent polystyrene polymerized in a potassium laurate solution with initial concentration of 0.1 molar. Soap micelles persist to a 20% yield of polymer. (*A*: Yield of about 5%; *B*: Medium yield, about 30%; *C*: Yield, about 70%.)


A
B
10,000A.
C

of the distribution curve. For each gram of oil, 1.8 g of water are used.

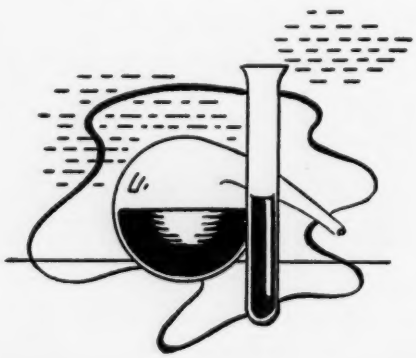
The molecules of oil diffuse into the water and from there into the middle of micelles, where they are solubilized. A water-soluble catalyst may be used, in which case a molecule of monomer (or mercaptan) in the aqueous phase is activated to form a free radical. This free radical enters the micelle and activates or stings one of the molecules of monomer present. In a fraction of a second, all the monomer in the particular micelle is converted into a polymer free radical, which continues its growth into the aqueous phase, thus robbing other micelles of their monomer, which diffuses out of them. Suppose that the sum of the molecular weights of the oils in the micelle is 4,000, then the molecular weight of the rubber molecule formed may be 250,000. This is so large that it not only converts all the soap of the individual micelle, but that of other micelles, to soap adsorbed on the coiled-up polymer molecule. This molecule aggregates with other rubber molecules, initiated in other micelles, to form a rubber or polymer latex particle (Fig. 8).

A remarkable fact now emerges, however. At low yields of rubber the polymer particle has dissolved 2.5 times by weight as much monomer as polymer, so the latex particle is truly a monomer-polymer particle. The monomer in this particle polymerizes, and more monomer diffuses in, but the ratio of monomer to polymer (M/P) continually decreases.

The diameter of the polymer particle, when fully grown, is of the order of 1,000 A, which is only one tenth that of the monomer droplets, so the monomer of one droplet forms *ca.* 1,000 polymer particles. This increases the area, so the amount of soap adsorbed from the solution on the particles is increased. This increase in the amount of soap which disappears from the water is so large that at a 20 per cent yield no micelles are left to initiate new rubber particles. The result of the disappearance of soap from the aqueous phase is that most of the rubber (monomer-polymer) particles are produced before the yield rises to 10 per cent.

The result of all this may be summarized as follows. Almost all the rubber particles have their birth in micelles, but less than 1 per cent of the rubber is formed in them. Over 99 per cent of the rubber is formed in the rubber particles themselves, by polymerization of the monomer which they dissolve. The suspension, or "latex," is stabilized by adsorption of a monolayer of soap around each polymer-monomer droplet. This causes *micelles* to disappear from the water. The *emulsion droplets* disappear at a yield of about 50 per cent, so at that yield the same weight of monomer as polymer is present in the latex particles. Thus soap is of extreme importance, since it supplies the micelles in which the rubber particles are formed, and in addition stabilizes both the emulsion of monomers and the latex.

Obviously the mechanism by which other polymers than rubber are produced in emulsion from relatively insoluble monomers is the same. In the production of what is known as cold rubber the micelles play the same general role. However, a catalyst which is soluble in oil as well as in water is often used, so there is more chance for the formation of the initial free radical in the monomer of the micelle. Also, the catalyst is in general more active, even at the lower temperature of polymerization. Thus more polymerization particles are initiated and the rubber particles produced are smaller. Commonly 10^{15}–10^{16} particles per cu cm are formed by either method, so the number of polymer particles produced annually reaches truly astronomical proportions.



Discovery by Illustration

HANS ELIAS

Educated abroad, Dr. Elias left his native Germany in 1934 to do research at the University of Padua, the Institute for Educative Cinematography (Rome), and the Athenaeum Pontificum Lateranense (Vatican State). Since coming to this country in 1939 he has worked at Harvard, Middlesex Veterinary School, and for the USPH in Atlanta, Georgia. In September 1949 he became assistant professor of microscopic anatomy in The Chicago Medical School.

THE nineteenth century was extraordinarily rich in discoveries in morphology and geometry. In the twentieth, both morphology and geometry have fallen into contempt. Scientists are no longer interested in shape. Biologists have become physiologists, mathematicians have become analysts. The biologist, of whom alone we shall speak here, will tell you that shape is unimportant, that only function matters. This may be true, but it should be remembered that the shape is the skeleton for the function. No organ would function as it does if it had not its specific structure. The comparison of the shapes of organisms, moreover, is the key to the history of their evolution.

How, then, was it possible that morphology should become despised? I venture to guess that the advent of photography influenced the neglect of visual observation, since it appeared to free the observer from the chore of drawing, though drawing is a potent force of factual observation. And, through the neglect of this shape-revealing force that drawing is, shape itself has fallen into oblivion.

The modern biologist sees something, or imagines that he sees something, in the microscope and rushes to the camera to record it photographically. The resulting photograph practically never contradicts the observer's subjective opinion, especially at high magnification, because of the limitations of depth of focus and of resolving power inherent in the optical system. High-power photomicrographs are usually hazy and in most cases cannot show the spatial relationship of parts of the preparation that lie above one another. This, however, is not the greatest weakness of photography. Its worst feature is that it fails to compel the observer to study carefully each little detail of a preparation. (Incidentally, the employment, by a scientist, of an artist to draw the illusrations for his publications may have identical consequences.)

The following cases will illustrate these points. In all of them, the effort to illustrate pictorially "well-established facts" led to their unexpected and unwanted reversal.

The chromatophoral "syncytium." In 1936 it was generally believed that the processes of the ramified pigment cells (chromatophores), and particularly of those which contain a black pigment (melanophores), in the skin of amphibian larvae, anastomose with the processes of their neighbors. Fischel affirmed most emphatically the existence of the syncytial network of the epidermal melanophores in tadpoles. His theory was upheld by Ginsburg, Goubeaud, and Elias. In studying the development of chromatophores in *Bombina variegata pachypus,* one is impressed by the density of the epidermal melanophore network. Its syncytial character appears to be obvious. In order to prove the presence of a syncytium, however, it was found necessary to trace at highest magnifications individual processes, from their origin in one melanophore to their insertion in another. Cell outlines were traced with a pencil and a camera lucida, and, to the utter surprise of the observer, it was found that each single process ended blindly. In many weeks of observation, not a single melanophore spur was encountered connected with more than one cell. However, the processes of one melanophore frequently cross the cell bodies of other melanophores and run for long distances alongside them and in contact with them. In this way the appearance of a syncytial network is evoked. The analysis of this complicated network was possible only through drawing. The observer who draws every detail with a sharp pencil must continually focus up and down, a procedure impossible when making photomicrographs. Had the photographic camera been used instead of the pencil, we should probably still believe in a melanophoral syncytium. Figure 1 is a reproduction of a high-power drawing of this alleged syncytium, the individual cells shaded differently and revealing their individuality.

Drawing as such is not only essential, but the observer must personally execute his drawings. The case of Fischel is an example: As his paper was re-examined, it was found, again with surprise, that the drawn illustrations gave no evidence of a syn-

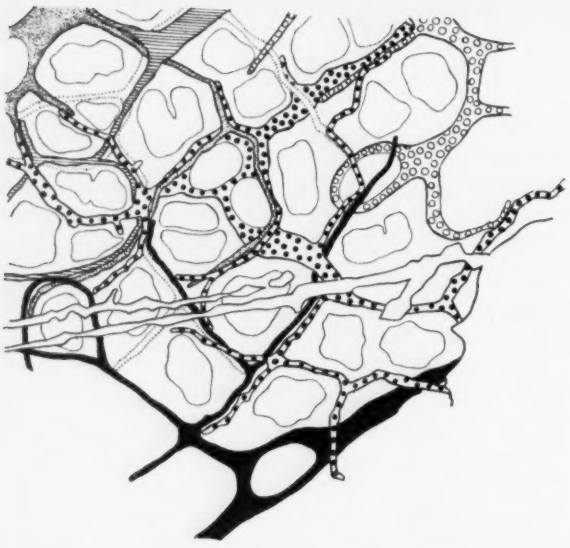


FIG. 1. Camera lucida tracing of epidermal melanophores of a tadpole of *Bombina variegata pachypus*. The overlapping and contact of neighboring melanophores can simulate, at lower magnification, and also at high magnification but less intense observation, the existence of imaginary anastomoses. This drawing shows, however, the individuality of the cells.

cytial arrangement. Instead, they furnished additional proof of the individuality of the epidermal melanophores. Fischel's illustrator, Mr. Kasper, had observed correctly, but he had no part in the interpretation of his drawings and in writing the text. Had Fischel personally drawn his illustrations, he would most probably have found the correct interpretation immediately. Yet the idea of the syncytium was still fascinating. Even if epidermal melanophores were nonsyncytial, the dermal and subcutaneous xanthophores, erythrophores, and leukophores *might* be syncytial. Again, hoping to be able to confirm this idea and looking into the microscope at the highest magnification, I had the distinct impression that at last I was able to confirm the syncytial nature of these networks that Goubeaud had so convincingly described. But alas, employing the same method of tracing the outlines of these highly ramified and interwoven cells with pencil and camera lucida, I established their individuality. Is not drawing, then, a much more objective and reliable source of information, in some cases, than the photographic plate, which allegedly never lies?

The superficial glands in the proventriculus of birds. In teaching histology to students of veterinary medicine, one becomes acutely aware of the lack of a textbook or even a reference work in veterinary histology. Thus, in order to provide some printed material for the students, data on the microscopic anatomy of domestic animals were compiled from the existing literature, and these compilations were printed chapter by chapter in the school journal. In order to avoid difficulties with copyrights, and also out of love for photography and drawing, new illustrations were prepared of what were thought to be well-established facts. When we came to the chapter on the stomach of domesticated birds, a curious thing was noticed.

The proventriculus, or glandular portion of the stomach, of the chicken had always been described as possessing two layers of glands: large glands in the submucosa and small, simple, tubular glands in the mucosa, close to the lumen. Illustrations in the literature seemed to bear out this fact. Foust's "Anatomy," in Biesser and Devries' *Diseases of Poultry*, contains a beautiful photomicrograph of a cross section of the proventriculus, showing numerous neat longitudinal sections of simple tubular glands. By photographing a comparable section (Fig. 2*A*) the same result was obtained. But one thing was puzzling. These longtudinal sections of tubular glands were all so neat, so exactly axial, as if they were diagrams of tubular glands in books. Why should every section turn out so perfectly? Something must be wrong, since tangential, oblique, and transverse sections of these supposedly tubular glands were totally absent.

This indicated that they could not be tubular. To determine their true shape one had to cut sections perpendicular to the ones observed; i.e., tangential sections of the proventriculus wall had to be made. For this purpose, the proventriculus was cut open and spread out flat, revealing a pattern of concentric furrows about the openings of the deeper glands (Fig. 2*B*). This pattern, which had already been seen in 1807 by Home, was an indication of slitlike, or sulciform, glands in this layer. Tangential sections (Fig. 2*C*) confirmed the slitlike nature of these glands, which concentrically surround the ducts of the submucosal glands. The superficial glands are, therefore, not tubular. They are not cylindrical structures, but flat. In whatever direction a flat structure is cut, the section will appear narrow and long (except if the section is tangential). Thus, through the sheer desire to prepare an illustration for student use of a supposedly well-established fact, a new and hitherto unknown morphological type of gland was discovered.

The cell "cords" in the liver of man and other mammals. The cases of alleged cords of cells in the zona glomerulosa of the adrenal cortex of the horse, which were found to be plates, and of "cords" of

the zona fasciculata, which was found to be a continuous parenchyma pervaded by cylindrical sinusoids (again, observations made in the process of preparing illustrative material for veterinary students), could be cited. These observations on the horse adrenal are perhaps of minor importance and not different, in essence, from what has just been said. The story could end here, except that the discovery of the laminar structure and of the vascularization of the mammalian (including, of course, the human) liver happened to be associated with film production. In the eyes of the universe, I think, the chromatophores of tadpoles and the proventriculi of chickens are just as important as the livers of men, but in this account the story of the human liver will be the main topic considered.

During the preparation of animation sequences for a motion picture on the life cycle of the blood fluke *Schistosoma mansoni,* a greater part of which is spent in the human liver, it appeared that major phases of schistosomiasis could not be understood without a thorough knowledge of liver structure. From my histology courses I remembered that, together with osteogenesis, the liver had been the most difficult subject for my students to understand.

FIG. 2. *A*: Cross section of proventriculus of chicken. Near the lumen are apparent longitudinal sections of imaginary tubular glands. At the bottom are seen the large, parietal glands. A duct common to several parietal glands is seen piercing vertically the superficial layer of alleged tubular glands. *B*: Surface view of mucosa of proventriculus of chicken, showing the sulciform glands (*sulci proventriculi*) surrounding the exits of the parietal glands. *C*: Tangential section of wall of proventriculus of chicken. The section reveals the fact that the alleged tubular glands are in reality slitlike (sulciform) and that many of them are arranged concentrically around the ducts of the parietal glands.

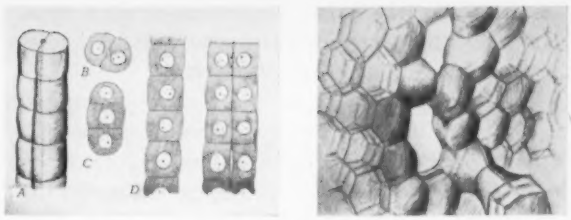


FIG. 3. Left: A liver cord and various theoretical possibilities of sectioning it. The least probable section (*D*) is the most frequent form found in slides. The two most probable sections, cross section (*B*) and oblique section (*C*) are almost never found. From this discrepancy between the cord concept and the histological slides one can conclude that cords do not exist in human livers. Right: Diagram of a liver plate. Bile capillaries run in intercellular spaces and some on the outside of the plates. The irregular plates are frequently perforated. Cell bridges connect the plates with their neighbors in some cases. It can easily be recognized that a section perpendicular to this plate will appear in a slide like the theoretical section (*D*).

I decided, therefore, to prepare a film strip on the architecture of the liver, as an introduction to schistosomiasis. The film strip was based on information available in standard textbooks, and the structure of the liver was shown in stereograms very similar to those I had drawn on the blackboard year after year.

All textbooks of histology (except Sharpey-Schafer's) agreed that the liver was built of curved, anastomosing cords of hepatic cells. Each cord was two cells thick and through its axis ran a bile canaliculus (Fig. 3, *left, A*). The cords were wrapped in sheaths of flattened cells of an endotheliumlike nature, and these cords, with their wrappings, were immersed in blood spaces called sinusoids. This is an arrangement difficult to visualize. I had tried to help my students understand it by asking them to imagine that their legs were liver cords, that they were wearing trousers, and standing in a pool of blood. The trousers represented the sinusoid walls, the spaces between the legs and the trousers, the lymph spaces of Disse. The cords are arranged, allegedly, radially around the central vein of each lobule.

With the help of Cineffects, Inc., a film strip was prepared. It consisted of stereograms, emphasizing the three-dimensional aspects, and was based on the old cord concept. This film strip was shown at the convention of the American Association of Anatomists in Montreal in 1947 in the presence of several specialists on liver histology. It was desired to have all mistakes corrected before releasing the film strip for teaching. The specialists were asked for their criticisms. They all agreed that the presentation was essentially correct and represented the true structure of the liver. Only one man in the

audience remarked that the sinusoids were drawn too large. This event shows how well established the cord concept of liver structure was.

It was decided to make the film strip ready for release; but, in order to increase its teaching value, some photomicrographs of microtome sections were to be incorporated so as to provide a transition from the stereograms to what the students would see in the microscope.

"This should not be too difficult," I thought, and began looking for adequate sections in my slide collection. For two evenings I looked at slide after slide; I went to the Anatomy Department of a near-by medical school and looked at all the liver slides in its histological collection. I still could not find the tangential sections of liver lobules containing many cross sections of liver cords. These should be groups of two cells, each with the cross section of a bile canaliculus between them (Fig. 3*B*), the bicellular group surrounded by endothelium and blood. According to the classical concept, about one fourth of the area of each liver slide should show just that. On the other hand, it seemed to be much too easy to find numerous longitudinal sections of liver cords (Fig. 3*D*). If one considers how difficult it is to find in a microslide a longitudinal section of an assuredly cylindrical structure, the fact that everywhere in liver slides he finds many longitudinal sections of highly tortuous and very thin cords, or trabeculae, becomes a very suspicious circumstance. Furthermore, these long rows of cells in the section are only one cell wide, whereas it had long been assumed that the liver cords are two cells wide. Also, oblique sections of liver cords (Fig. 3*C*) and bicellular, longitudinal sections, which could be expected (Fig. 3*E*), were absent.

Obviously, something was wrong with the hepatic-cord concept. Since the long, tortuous rows of cells are not the only structures visible in histological sections—larger patches of polygonal cells, and single cells, occasionally occur—I could only conclude that the mammalian liver consists of anastomosing, curved plates which are one cell thick; that those plates form the walls of more or less cylindrical spaces in which the sinusoids are suspended; and that uni- or bicellular bridges sometimes connect opposite walls of such spaces. The right portion of Figure 3 is a stereographic view of a hepatic plate. The location of the bile capillaries between the cells and at their free surfaces is shown, as well as an interlaminary bridge.

These theoretical deductions from the examination of single sections were submitted to critical tests, and all available methods of three-dimensional reconstruction were employed to verify this new theory. The theory was found to be correct, and the results were published (*Am. J. Anat.*, 1949, **84**, 311–34; *Science*, 1949, **110**, 470–72; *Am. J. Anat.*, in press). The new concept of liver structure that emerged from this attempt at three-dimensional visualization represents a complete reversal of the old theory: whereas the classical histologists believed in cylinders of hepatic cells, surrounded by blood, these recent observations showed that the blood-filled sinusoids are cylinders of blood cells, surrounded by hepatic parenchyma.

During the course of completing the film strip on the normal histology of the liver, the terminal branching of the intrahepatic portal vein and of the hepatic artery had to be pictorialized. I found that practically nothing was known on this subject. Textbooks had covered these gaps in our knowledge with verbalisms which appear to be satisfactory when read, but which cannot be pictured in an unequivocal manner. Since no precise information on the terminal distribution of these vessels could be obtained from the literature, nature had to be observed, always with the view of preparing accurate pictures for student use. These studies resulted in a detailed analysis of this long-neglected topic.

The results of these experiences may be summarized as follows:

Certain phenomena of nature frequently appear to be conclusively solved, because they seem to be well described verbally, and sometimes they even seem to be documented adequately by means of photographs. However, the effort to produce accurate drawings (resulting from the knowledge of the physically insurmountable limitations of optical systems and photographic emulsions), and the necessity for showing phenomena in a concrete form on a screen for teaching, compel us to observe nature more acutely. This discipline of illustration and visualization is a great force in the process of discovery. Scientific drawing which, during the nineteenth century, was almost universally handled by the investigators themselves, has contributed greatly to the advancement of science in the past. The production of visual teaching materials, atlases, charts, models, motion pictures, film strips, lantern slides, and perhaps other devices, if handled by personnel trained both scientifically and artistically, may contribute more and more to scientific progress in the future. We may also witness in this process a renewed interest in the shape of living things, and the recognition that knowledge of structure is one of the cornerstones for the understanding of function.

The Development of a Research Interest

HARLEY J. VAN CLEAVE

A specialist in parasitology, Dr. Van Cleave (Ph.D., Illinois, 1913) has spent most of his academic life at the University of Illinois, where he has been research professor since 1948. His article is based on his address as retiring vice president and chairman of Section F, delivered at the Zoologists' Dinner at the Christmas 1949 Meeting of the AAAS in New York City.

WITH something more than a dozen separate sections within the American Association for the Advancement of Science, a vice presidency is somewhat comparable in its significance to the same office in a large corporation or in a metropolitan bank. It has long been at least tacitly agreed that the most obvious obligation of this small cog in the intricate machinery of the Association is the preparation and delivery of a swan song. Consequently, I began to mull over what I might say, or be expected to say, just as soon as an intimate friend in the distant frontier state of Nebraska congratulated me by letter upon my election to the vice presidency of Section F. Incidentally, my informer has absolutely no official connection with the administration of the affairs of the Association. I have no way of telling whether he exercised some occult power of divination in ascertaining the fact, was advised through the illicit dribble of normal leakage from channels of classified information, or, as is so common a practice with scientists, mistook a bare chance for a reality. Regardless of the interpretation to be placed upon the grounds for his warning, I was deeply grateful to him for the advance information, since it gave me something like three weeks of academic leisure in which to consider all sides of the matter before I received the official notification of my election.

My first reaction was a feeling of deep appreciation for the personal friendships that had made my selection possible. These seemed inevitably the underlying explanation of my election, since the tired, old-fashioned lines of my morphological and taxonomic researches cannot boast the glamorous popularity of many of the more recently exploited fields of investigation. Due realization of this last situation imposed some serious problems concerning the nature of a suitable address to fulfill the formal obligation implied by my acceptance.

It seemed obvious that the address should be concerned with research. Among the special problems that have interested me personally there did not seem to be one directly suited for such an occasion. It therefore appeared appropriate to talk about research rather than attempt to dress up a fragment of my own investigations for a general audience.

It is self-evident that to successive generations of scientists research carries widely varying connotations. To an early generation, purely descriptive morphology and taxonomic appraisal of the local fauna represented the first essential step in an understanding of the problems of living things. The place of an individual kind of animal in the economy of nature could not be discussed intelligently until some sort of distinctive name had been applied to it. Nor could functions be understood before the structures that participate in an activity were seen in their normal spatial relationships. Thus the foundation stones of taxonomy and morphology had, of necessity, to precede any more elaborate analytical study and understanding of animals, either as individuals or as communities. There remain frontiers where investigations at this level must still be carried on. Even today there are many areas of the earth's surface and many of the little-understood groups of the animal kingdom where competent work in morphology and taxonomy must of necessity precede the development of an experimental approach to zoology.

In the integration of zoological knowledge, there is nothing more pathetic or annoying than

an experimental or analytical study that ignores the taxonomy and morphology of the animals under investigation, unless it might be a study in taxonomy or morphology ignominiously oblivious of functional and habitat relations.

In recent years, zoology has been becoming progressively more specialized in its content, more elaborate in its organization, and more diversified in its aims and methods. It has proceeded along such diverse lines that there are but few common grounds where groups, or even individuals, may meet in a sense of mutual interest and basic understanding. Early in the days of the rapidly expanding horizons of biology, everyone felt impelled to listen with real or feigned interest and attention to the newest discoveries in cytology, in genetics, or in ecology. It was the mark of a cultured, liberal biologist to be able to discuss, intimately, the results of individual research programs. Even fifty years ago, zoologists and botanists held enough in common that they, as naturalists, met to discuss mutual problems.Today, within the individual subdivisions of zoology, the diversified interests have become so specialized that it takes the materialistic animal gratification of food to bring the varied brands and sects of zoologists together for an evening, and then as an anticlimax they, by custom, must listen to a speech, at the close of which they are not even granted the courtesy of open discussion or disagreement.

This unwonted freedom from direct assault which is extended to the vice president of the section when he delivers his address seems to have been an outgrowth of necessity. There is little of importance and there are few conclusions that could be presented to an audience of this sort without arousing the opposition of partisans who subscribe to divergent beliefs or interpretations. In open programs, where an investigator volunteers to air his thoughts, he accepts the personal risk of being confronted by hostile challenge and denial. That is one of the ways in which science progresses. Ideas and conclusions, as well as organisms, undergo a form of natural selection whereby the unfit and unsound are finally eliminated. When a speaker appears on a program by election, there is an implied immunity from attack, because otherwise chairmanships would have to be filled by the un-American process of conscription.

For many years I had listened, more or less attentively, to the delivery of vice-presidential addresses in a detached attitude of indifference as to how they came into being. The fragments I could recall seemed to have little in common. As I thought back over the scores of Section F addresses which I had either heard or read, I could remember some which were brilliant, a few which to me seemed frankly stupid. Some represented masterly summations of rich experiences in original thinking, and a few were obviously put together from odds and ends, merely to satisfy an obligation. Some were delivered with enthusiasm and in the compelling voice of confidence and authority, others in indifference or in an apologetic utterance of uncertainty and lack of conviction. Lapsing into the long-enforced habit of assigning grades to intellectual efforts, I find that, after all, vice presidents are not so different from other human beings and that their addresses fall within the series of grades ranging from superior to questionably passable. Members of a society who from loyalty or eternal anticipation attend formal meetings regularly are extremely fortunate that chairmen who fail to deliver a satisfactory address are not required to repeat in the expectation of raising their standing, as is the indefensible custom in our educational institutions for the treatment of indolent and incompetent students or intellectual derelicts who are compelled to repeat failed courses.

Having never seen a vice president publicly lynched or shot because he did not come up to the expectations of his constituents, I formally accepted the honor and set to work on the formulation of an address. Reassured by the length of time available for its preparation, several trial flights of rhetoric were attempted, but, after the first explosive burst of enthusiasm wore away, each of these seemed to sink gradually to the lower levels of attainment mentioned above. From the undulating succession of highs and lows achieved in the sundry starts, a series of observations has finally been put together.

I

Naturally, my first inclination was to develop a philosophical approach to some aspect of parasitism, but most of my friends are already familiar with my views in this fascinating field. I was afraid that those who are not would feel that I was merely attempting to exploit the field of my personal research interests and might find the topic neither enlightening nor interesting. Consequently, I have chosen a more abstract topic which has direct importance for all zoologists, since, as investigators and as teachers, we are all concerned with the problems of developing a research interest.

There are at least three rather sharply distinguishable aspects to this problem. First comes the realization of a personal interest in research.

This may derive from an honest professional allegiance to science as a career, or it may be stimulated by failure to secure admission to a medical school. The time of its appearance varies greatly. The most extreme instance that has come to my attention was the case of a colleague who upon entrance to the preparatory school announced the subject of his doctoral dissertation. For some, the burning desire for research is awakened by the announcement in the university calendar specifying the last day for filing research topics. Close on the heels of this awakened interest follows the choice of a field of investigation. Inspiration by contact with capable instructors in basic courses who instill in their charges a desire for knowledge is probably the most normal directive influence in the life of a student. But accident of location and personal associations may either direct or limit the choice. Finally, after an investigation is under way, there arises the problem of the attitude toward the evaluation of the established program by colleagues in other fields. The first two of these problems are largely matters of choice by the individual, although in many institutions students are literally shanghaied into specific programs, regardless of their personal desires or inclinations. Especially in the past, some advisers of graduate students are reported to have shared the zeal of the proselyting missionary. They rescued the prospective investigators from the sad fate of falling into one of the less-respected fields of research and thereby in the eyes of administrators built reputations for attracting students. Naturally, the free choice of an institution and of a sympathetic adviser are the most potent cures for this situation. The third element is wholly outside the control of the individual, since it reflects the administrative biases and popular prejudices for passing fads in research. These tendencies at times become so extreme that an established leader in a field may be branded as "deadwood" by upstarts because the old fogy is unwilling to abandon an unpopular field of endeavor in which he has invested a lifetime of labor, in response to the stampede toward the firing line where new programs are unfolding. We sometimes forget that newness is no guarantee against the inroads of dry rot. There are some zoologists who have departed so far from the traditionally basic fields of morphology and taxonomy that they would feel these are no longer legitimate areas for investigation. Fortunately, however, at the present time there are many leaders in the fields of genetics, cytology, physiology, and ecology who appreciate fully the significance of morphological and taxonomic work as background in the interpretation of observations and experiments in the new aspects of old problems.

The invidious distinctions and discriminations between pure and applied science, which occupied so much attention a generation ago, have in large measure been stilled. There was a time when students of so-called pure science belittled all investigations which held any promise of practical application. Now the emphasis seems to have shifted in the opposite direction. The importance of understanding human and animal disease and health and of increasing food supplies became particularly evident during the recent war, and false concepts of superior races have awakened as acute interest in the fundamental facts of genetics.

Anyone intimately familiar with some branch of zoology can readily furnish instances wherein the pure and the applied or practical merge insensibly into one another. Thus the imaginary line between these two areas of knowledge has been largely erased, and the student can enter either field of research without feeling undue pressure, since practical benefits may result from the study of a purely theoretical problem and a distinctly economic study may reveal fundamental facts.

Before launching into a discussion of the main subject, it would probably be expedient to examine, to some extent, my personal transition from calm and unquestioned acceptance of things as they are to an attitude of honest inquiry.

When I stand before the impersonally frank mirror of the written page to view and review my own thoughts and opinions regarding a changing attitude toward authority, I am often shocked at the inconsistencies that have developed and some that have persisted in my own viewpoints. I am aware of changes, but I cannot adopt the blissful interpretation that change necessarily means progress. Blind and dumb respect for the written and the spoken word was shaken with the first attempts to solve problems independently. Even then there remained a certain degree of respect for authority and some feeling of resentment against brash and immature opposition toward reputably recorded observations and generally accepted interpretations based upon these observations.

I well remember the day when a surge of appreciation for independent thought and burst of enthusiasm shook loose the last remaining remnants of resentment toward open attack against things as they are supposed to be or threaten to become. This milestone in utterly independent thought arose in the midst of a session of Section F. The speaker was that master of clear thought

and fearless and forceful expression, the late William Morton Wheeler. As I stood in the doorway at the rear of the room, I was uncertain as to the relative returns to be gained from listening to a vice-presidential address or the more delightful and relaxing opportunity of visiting in the hallway with friends. The powerful and picturesque words of the speaker dispelled all indecision, for I soon found myself translated to a new level of emotional experience.

On that December day in Chicago, in 1920, I experienced a new evaluation for, and a full-fledged appreciation of, courageous and confident opposition toward ideas which meet general popular approval because of lack of honest public appraisal. The speaker dressed his ideas in such charming simplicity and sincere understanding of human nature that an emotional conversion to his points of view came easily. Previously, I had dogmatically accepted the hypothesis that organization was the highest expression of human intellect and of social and scientific relations. Professional organizers had repeatedly voiced this opinion, so why should not all loyal members accept it as ex cathedra? The privilege of listening to a man with keen intellect, unusual breadth of experience and contacts, and superlative diction provided new standards of evaluation. To hear him challenge the validity of the growing tendency toward superorganization gave an entirely new basis for appreciating how easy it is to accept and conform to rather than challenge a widely supported idea.

In that address, among other things, Professor Wheeler questioned the expediency of attempting to organize such things as courtship, or a vacuum, or research. The first and last of these were of enough immediate concern to me that I followed his entire discourse with intense interest. Fairly recently I had experienced a seemingly highly profitable adventure in courtship without being conscious of any shortcoming attributable to lack of organization. Besides, I had started to feel that I was beginning to attain some measure of success in a purely individual research program. Blissful ignorance of what I might have accomplished if I had become a cog in an integrated research program has continued to this day to keep me fairly well satisfied to proceed without the presumed benefits of external organization.

My training in the field of physiology and in the explanation of the control of emotions is not sufficient to enable me to understand these transitions in attitudes and interests. I have never known whether they were merely signs of a natural progressive change in a maturing behavior pattern or whether they might be attributable to some hormonal control. Perhaps the stimulus of the words spoken by Professor Wheeler was all that was needed to set off a chain reaction, transmuting the attitude of stolid conformity to one of mildly irritated rebellion against the inherent rightness of things as they are.

This metamorphosis in attitudes tended to engender confidence in the belief that no field of research is, in itself, either superior or inferior. The development of a research interest may be viewed historically as a record of shifting emphasis in science or as an individual problem which confronts every human being who crosses the scholastic threshold to enter any research project. Both aspects of this two-horned theme will be treated briefly.

II

In the three hundred years since the early colonization of America, science has become so highly diversified and specialized there is little wonder that the inquiring mind of youth today finds direction far different from the ambitions and motivations of scientists of generations past. In science, as in politics, it is regarded as old-fashioned and unprogressive to follow any line of inquiry not regarded as modern. Our concepts of science and of research have undergone phenomenal change since colonial times.

From our present-day experiences with fragmented bits of science, it is difficult to realize that a man like Thomas Harriott could have lived up to his characterization as "a man of wide culture, a skilled astronomer, a profound mathematician, the author of a standard treatise upon algebra, and a botanist, zoologist, and anthropologist withal." Moderns who recognize specialized experimental biology as the only field worthy of cultivation could find little which they would admit as research in Harriott's "particular narrative of all the beasts, birds, fishes, fowls, fruits, and roots, and how they may be useful." But knowledge at this level in 1590 formed the background of observations out of which individual research problems gradually emerged.

Likewise, in the colonial days, the scientific communications transmitted by the Governors Winthrop to the Royal Society of London had little that now sounds like research results. The Winthrops, with Mitchill, Garden, Clayton, and Rittenhaus, are random samples of the representative naturalists of the New World making elementary observations and drawing limited conclu-

sions regarding the American fauna and flora before modern science was born on this continent as an almost inseparable dependency of educational institutions.

Altogether too often there is a feeling that there is something inherently superior about researches conducted in an institution. The problem is almost wholly one of finances. I thoroughly disagree with all those who have cast aspersions upon the so-called amateur scientists. Our modern American system of education has almost wholly failed in instilling intelligent, participating interest in the graduates of colleges and universities in fields outside their narrowly specialized curricula. In the democratic state, politics, government, religion, law, are all supposedly the result of the composite thinking of its individual citizens. Although we have professional politicians, clergy, and lawyers, there are too few outside these specialized fields who take seriously their opportunity and obligation as individuals with a right to express opinions. In recent generations, it seems that we have been even less successful in the instillation of an innate interest in science in the hearts of students who are exposed to scientific courses. It is only after neighbors' rights to participate in such matters have been withdrawn by a police state that we begin to appreciate the importance of some of our neglected privileges. One of the truly significant rights in science is the freedom of the individual to select a field for investigation.

The emotional eruptions against scientific investigation which find expression in the so-called antivivisection blocs are examples of the damage that can be inflicted by emotional appeal in crusades against science. If the seeds of intelligent appreciation are sowed in the minds of all high-school and college students who pass through a science course, such organized obstruction would fail from lack of public support.

Our facetious and condescending attitudes toward the so-called amateurs in science are too much influenced by the connotation of that term in athletics. In sports, the chief criterion for an amateur is that he should not receive remuneration for his efforts. By this yardstick, a generation ago there were many amateur scientists, even in our best universities, since salaries did not exceed mere maintenance. At training tables colleges and universities could do that much for their athletic teams without endangering their pure amateur status.

It is very unfortunate that this warped concept of the amateur has so largely dominated the thinking in the field of science. In truth, every major branch of science has been greatly advanced by the efforts of amateurs or noninstitutional workers. It is not necessary to do more than mention the names of Darwin and Mendel to illustrate the point. I regard it as one of the most unfortunate failures of American education that we have produced so few men in this category as compared with the products of British and continental European universities.

Today science and its research results have passed far beyond comprehension of the mind of average training and intelligence. With the ever-increasing specialization of fields, it takes some agency like the AAAS to integrate and coordinate the diversified fragments. We used to think of zoology as a fully consistent field, but under the impact of intensive researches, especially during the past two generations, it has exploded into a vast array of separate parts with varying degrees of evident relationship.

Regarding this diversification and complexity as evidence of progress, we are inclined to lull ourselves into a state of satisfied complacency. Yet within our own science we have long been aware of the sad fate of some species for which specialization and development of complexity have carried in their wake a threat of disintegration and extinction. The long, proud march of the dinosaurs and the nautiloids toward greater specialization and higher complexity imposed limitations which culminated in extinction. Many highly specialized animals have escaped the martyr's path to extinction but have become so restricted in their biological and physical relations that they can exist only under very narrowly specific surroundings. I refer especially to the intricate biological adaptations of certain parasites, restricted to one or more different species of hosts. Often these adaptations are attained in such minute detail of dependence that the fate of the individual parasite, as well as continuation of the species, depends upon the full satisfaction of all aspects of the adaptation. It is not uncommon for these adjustments between a parasite and its obligatory host to become so perfect that the parasite loses many of its structures and functions. I realize that this statement savors of Lamarck's doctrine, but the facts imply no necessary commitment of any specific causal explanation. It is widely recognized that in their adaptations to their environment parasites lose many of the morphological features distinctive of their closest free-living relatives. Blinded by the obvious organic simplification of parasites, zoologists, with naïve assurance, have fairly regularly

decreed that these highly specialized organisms are degenerate. But, in the past, man has often misbranded progress as degeneracy and has just as often erred in the opposite direction when he has mistaken degenerate tendencies for marked advances. It is easy to forget that progress may be in any direction and implies advancement toward a goal, with no necessary implication as to the direction of the advance.

III

Toward what sort of a goal are we advancing in biological research? It is obvious that in our educational system we have not been very effective in training students to have an intelligent interest in research or to formulate a definite research program. It seems probable that the mass production with which graduate work is conducted at the present time may be responsible for some part of this shortcoming. Under these circumstances it is easiest either to throw the beginning research student upon his own, to follow an ill-chosen problem, or to assign him a small, isolated fragment of his director's research project. Under either of these plans only the stubborn survive, and by little-guided steps they come to an independent but possibly warped realization of the meaning and possibilities in research. There is all too often scanty individual inspiration offered by either of these methods. No member of the faculty can inspire twenty or more research students at the same time and offer intelligent direction to their efforts in addition to a full program of lectures and laboratory teaching. Such pressure is wasteful of the efforts of the instructor and destroys the hope in the souls of his students.

In these days, when subsidies are so widely and liberally spread, it might be dangerous to speak against them. It is my honest opinion, however, that certain dangers are inherent in the extent to which graduate study is subsidized at the present time. In most of our institutions of higher learning, there has been a phenomenal increase in enrollment of graduate students since the war. By personal inclination and by administrative design, the flow of graduate students which was interrupted by the war has been speeded up. The majority of these students are enjoying stipends as fellows or as graduate scholars, and many are gainfully employed as graduate assistants in teaching and in research laboratories. The backlog of vacancies that accumulated during the war period was so great that it may be some time before there is any assurance that the shortage has been overcome. In the meantime, the demand has far exceeded the supply of competently trained applicants, with rather disastrous results on standards. Fundamentally, the subsidization of advanced study is extremely desirable, but, like many benefits, it carries certain objectionable features, some of which may be eliminated when social, educational, and economic equilibrium becomes established again.

In the meantime, the war has awakened a general public appreciation of the desirability of a college education, and the effect upon the educational institutions has been greatly augmented by provisions of the various GI bills. The physical plants and instructing corps of all colleges and universities have been taxed to the utmost. This situation developed at the time when the supply of fully trained personnel at the doctorate level was at its lowest ebb due to the operations of Selective Service. To meet the unprecedented demands for teachers of elementary students, large numbers of graduate students had to be employed as assistants. Many of these were of a calibre that would not have been encouraged to continue with graduate study in normal times. Individuals who received such appointments were very commonly less concerned about entering any specific field of research than they were with profitable employment. Consequently many of them were willing to accept appointment in the institution that would provide the highest stipend. By this method, the institution selected the student instead of the time-honored custom of the student exercising a choice for an individual or a department strong in the field of his specialized interest.

Thus, by subsidy, the driving power of individual initiative on the part of the student in the selection of a research director has been largely lost. It was not unusual for the appointee to arrive on the campus before he even knew the names of men under whom he might be obliged to work. Staff members at both ends of the scale were penalized by this method. Those with high scientific standing, as well as those representing a popular field, or individuals with a reputation for leniency and charity, had their laboratories overrun with mediocre students. Conditions of this sort result in unavoidable repercussions against the university and the department. Furthermore, they impede the progress of science.

Unless the methods of selecting graduate assistants can be improved to ensure admission of only superior students with honest professional interests and individual initiative and motivation for research, science might be better served by return to former conditions. This is true even though the

return might mean the reappearance of the financially insolvent but intellectually independent graduate student who carefully selects a field and a director of his research before he invests his time and his effort in the training for a professional career. There is no inherent virtue in being a pauper, but subsidies in education may carry the same tendency toward loss of individual initiative that has characterized their effects on business and on agriculture.

When a student is contemplating the entry into a life of research he might profitably stand back and evaluate the entire field of his science objectively. In so doing he could profit by trends which have developed in the past. A special field which looks promising today may be less attractive ten years hence, whereas an older one which seems almost static may offer unusual opportunities for adequately trained students with initiative. Some of the fields in zoology are advancing so rapidly that they seem especially attractive to the beginner. Yet he should recall that there is an inevitable lag between the time when he decides to enter a given field and the time when he is ready to embark upon his specialty after serving the almost inevitable apprenticeship in elementary teaching. By the time he is ready to develop his independent professional career, the field of his choice may be overcrowded. This is not too forbidding a situation, because in every field there is always room for the exceptional individual at the top. Anyone familiar with the history of zoology in the past fifty years can readily recall one or more instances of avenues of highly popular research which have slowed down materially or have become less prominent than are some of the less spectacular developments in science.

Although programs of research in zoology are not static for long periods of time, neither is it essential that any individual who begins in one line of investigation must stick with that single field. Some, through lack of imagination, or obstinacy, or feeling of personal responsibility, adhere tenaciously to a program that they have started and evade the call to fields which have apparently greater popular appeal. But divorce from a problem to which the individual seems to be wedded does not even require change of residence. Furthermore, any soundly trained individual can readily adjust himself to new projects. In fact, one or more secondary research activities commonly offer diversion and a type of intellectual recreation with new professional contacts when pursued along with a major field of investigation. The history of zoology is filled with radical transformations of individual interests. Darwin was a taxonomist before he turned to the formulation of his concepts and explanations of evolution. Morgan was well established as an embryologist before he championed the momentous and contagious program in laboratory analysis of *Drosophila* genetics. Jennings was an authority on rotifers and teacher of comparative anatomy before he embarked upon his investigations on heredity in *Paramecium*. Davenport was the leading American student of biometry prior to the development of his consistent studies in eugenics and compilation of data on human evolution. This list could be expanded indefinitely, because every new field is, with few exceptions, championed by individuals whose earlier interests centered in other and usually more nearly classical lines of endeavor.

Regardless of the field of his choice, the biologist's endeavors will almost invariably conform to the same curve that other life processes follow. The enthusiasm of youth is often marred by either hero worship or by cynicism resulting from the first recognition of the errors and faults of contemporaries and predecessors. Normally, this is but a transient phase of adaptation in the acceleration of the life curve. Attainment of modesty and humility is normally a mark of maturity of attitudes. Retrospect becomes more apparent as the peak of the curve is passed, and the less we say about the final period of senescence the better. Regarding this period, it is all too common for the attainment of distinction in some special field to bring public adulation which causes the brilliant individual to be regarded as an oracle in matters of politics, religion, and social problems, even though these may all be entirely outside his field of competency.

Some of our liberals argue for freedom (when they mean license) in education and express opposition to efforts toward restriction within our institutions of learning of the impartial analysis and evaluation of literary, political, economic, social, scientific, and religious tenets. I rather expect that few honest thinkers, even among those who are not regarded as liberal, would object to the free analysis and evaluation of any tenets. The thing to which many of us offer objection is the attitude which seems to desire special privilege for un-American ways and doctrines, to the detriment of American institutions and practices. In science, we have had altogether too many individuals who like to regard themselves as liberals and in that guise become supporters of subversive doctrines and align themselves with questionable organizations.

Unfortunately, some of these individuals who think of themselves as liberals are in a position to express their biased opinions as the voice of the entire organization. Personally, I am unliberal enough to hold a feeling of deep resentment against the proposal that certain restrictions are un-American. It appears to be as much of a crime against nature to check an infection as it is to oppose the inroads of destructive and demoralizing influences in our political and educational bodies. The same individuals who regard themselves as spokesmen of the American people are often the officers in our organizations who by virtue of their positions create a bias for or against particular lines of research.

To the sincere investigator no field of research is intrinsically superior to another. In revealing the facts of the living universe, the structure and classification of a worm from Madagascar may be as important as an experiment with isotopes.

The scientist who is really interested in research is but little influenced by public appraisal. He is driven by forces from within that impel him to devote his life and energies to investigation. If he has a program already under way, an assigned task may be carried out mechanically but never with the enthusiasm with which he follows his own problems. To him, research is an essential part of his existence. The significant returns are not in the form of public or administrative approval or prizes but rather in the self-satisfaction of work well conceived and carefully done, bringing the personal gratification of accomplishment. Inadvertently, he may be drawn into the habit of the accountant, enumerating titles of articles and counting pages of print, but within his own consciousness he has other, less grossly quantitative, standards by which he evaluates his own results and integrates them with the researches of his colleagues.

The individual who is harried by the perfectionist's yearning for completeness cannot delimit individual problems to his own satisfaction, because he is too keenly aware of the fragmentary nature of all his efforts. On the other hand, the realistic investigator follows a line of research that may be either extremely narrow or deep and broad. In either case, he has full conscious realization that his observations and conclusions are but facets of the many-sided body of truth and reality. Each new discovery fits into other facts and interpretations and opens an ever-widening vista of new problems for solution. Even the repetition of a fixed procedure may reveal new relationships which give breadth to the narrowest and most confined lines of investigation if the effort is motivated by a sincere and competent interest.

In biological research there is often the prudish pretense that the observations upon which conclusions are drawn are purely objective. But, at times, it is evident that not all observable materials are of equal value in their application to the solution of a problem. Even the most elemental assertions may be warped in their application to preconceived conclusions. The following anecdote illustrates how different interpretations may be derived from the same set of facts.

The story is told of a visitor to the United States who traveled extensively through the various parts of our country. When given an opportunity to express his impressions of America, he professed grave concern over our obvious lack of attention to problems of personal sanitation. In all his travels across the length and breadth of our land, he had not seen a single delousing station. Observations of this sort are often a compliment, even though they may be spoken in contempt and in ignorance of more fundamental facts. A direct corollary to the traveler's remark would be that in the same travels he encountered no cases of epidemic typhus, no relapsing fever, and no trench fever, all of which depend upon lice for transmittal of their causal organisms to the human body. Taken out of their context, isolated observations often lead to erroneous conclusions. However, at times these lead to a fresh approach that may set up new tentative conclusions for extermination when the final evaluation of a phase of the problem is rendered.

IV

In the broad fields of the unknown in zoology, there are deep veins of truth still unexplored. In these there is plenty of opportunity for wide choice in establishing a field for research. For most of the older fields, new techniques and new interpretations make reinvestigations profitable. Between the well-established shafts of deep inquiries there are still many rich pockets of truth. Thin outcrops at the surface may often expand into broader seams after a little exploratory digging. In most areas there are but few nuggets remaining on the surface, waiting to be picked up by the casual prospector. The student unfamiliar with the art of prospecting will often overlook rich deposits in his interest to pick up fool's gold and the scanty grains from the worked-over trash heaps of former generations of scientists. Guidance of the novice in recognition of relative values is one of

the greatest opportunities of the sincere director of research who places the training of sound investigators and teachers above the advancement of his own research program.

The opening and developing of entirely new fields of research are largely fortuitous and unpredictable, depending largely upon momentous discoveries and unusual events. There is little that the individual or any group can do in directing major movements of this kind other than contribute momentum by keeping a shoulder to the wheel of scientific progress. The greatest promise lies in the contribution which the individual and the institution may make toward helping the exceptional student to find himself and to develop a consuming interest in research.

It is not often that youth is seriously influenced in the choice of a field of research by any feeling of social obligation or feeling that one field holds greater opportunity for human and social service. Consequently, it cannot be too strongly stressed that personal pleasure and satisfaction are at least as high motives in the choice of a field of research as are desire for acclaim or fame. Under the guidance of a wise director the student may be trained in recognizing and solving the unknown areas of science, and the return to both is greater than results from the assignment of a student to the tedious working out of the details of the director's personal program of research.

Neither overcrowding nor unpopularity of field of his choice need disturb the competent student of today or of the future. No matter what field he may select, there is always a place for the superior intellect motivated by the urge for honest work and desire for accomplishment. As with the leaders of ages past, he may attain distinction in the field of his original choice, or he may have the distinction of pioneering in a new program for the development of an entirely new research interest.

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The Gulf Stream*

A Brief History of the Ideas Concerning Its Cause

HENRY STOMMEL

The author, whose main field of interest is the dynamics of ocean currents, was an instructor in mathematics and astronomy at Yale for two years after his graduation in 1942, and since 1944 has been research associate in physical oceanography at the Woods Hole Oceanographic Institution. This paper was written largely during a visit at the Scripps Institution of Oceanography.

THE long history of the Gulf Stream from the time of its recorded discovery to the present —a span of 436 years—has been full of theorizing as to its cause. The observations I shall make will be restricted to those that have some documentary evidence—thus excluding a large body of speculation about Norse, Arab, and Portuguese navigators—and that have some bearing upon the theories that have purported to explain the presence of this great ocean current.

Early Ideas and Explorations

Although the island of Cuba was first circumnavigated in 1508, it was not until 1513 that the Gulf Stream (specifically, the Florida Current) was described by Ponce de Leon, who, sailing from Puerto Rico, crossed the stream north of Cape Canaveral and then sailed south to Tortugas. The current was so swift that Ponce de Leon's three ships were frequently unable to stem it (Herrera, 1601).

By 1515 Peter Martyr reports various conjectures about the Gulf Stream. His arguments are based essentially upon the principle of the conservation of mass and the tacit assumption that the current velocity is independent of depth. Martyr argues that the north equatorial current must (1) pile up large masses of water at the Brazilian coast, or (2) pass through some great straits or passages into the Pacific and thence round again into the Atlantic, or (3) be deflected by the mainland so as to flow back into the ocean (Kohl). The first possibility is ruled out, he claims, because the explorers of the Brazilian coast have never noticed such a piling up of water. The second possibility is doubtful, because it is the consensus among the Spanish navigators that the mainland is not open, but presents a continuous barrier to the westward flow. The third possibility is left by elimination, and as an example of such a deflection of the equatorial current by the mainland, the Gulf Stream, says Martyr, is a case in point.

The surprising accuracy of Martyr's ideas is accentuated by the raw nonsense that learned men of later centuries devised as explanation and description of the stream. Martyr is concerned only with the principle of conservation of mass. He does not seek a dynamical cause. And he has a very blessed gift: skepticism about his own ideas: "Thus we have made you partner of such things as they [his colleagues Andreas Moralis, Ouidas, and Diegas Colonus] have given us and written their diverse opinions. We will then give more certain reasons when more certain truth shall be known."

The westward flow of the equatorial current was usually attributed to the *primum mobile*—in some vague manner the general westward motion of celestial bodies across the sky drew the water and air of the equatorial regions along with it.

By 1519 the Gulf Stream was so well known that Spanish ships bound for America came by way of the equatorial current, but returned through the Florida Straits, following the Gulf Stream to about the latitude of Cape Hatteras and then sailing eastward to Spain. In this way they had favorable winds and avoided contrary currents over the whole voyage. The currents were less well known to the geographers who stayed at home, however: Oviedo (Kohl) describes minutely the New World coast but makes no mention of the currents.

These years mark the beginning of a period of intense activity in the western North Atlantic Ocean, of exploration of the coasts, and of the search for the Northwest Passage. Many of the early cruises in and about the Gulf Stream are chronicled in Kohl (1868) but need not concern us here. The navigators of various nations investigated the geographic extent of the Gulf Stream

* Contribution No. 503 from the Woods Hole Oceanographic Institution.

system. Among these we may number Sir Humphrey Gilbert, who first suggested using a deep-sea anchor to determine surface drift, Martin Frobisher, Ribault, and Laudonniere. Frobisher and John Davis made numerous observations of the Labrador Current.

A. Thevet, in 1575, attributed the Gulf Stream to the great rivers that flow into the Gulf of Mexico. It was many years later, when the transports of mass through the mouth of the Mississippi and the Straits of Florida were measured and compared, that the utter inadequacy of such an explanation was completely revealed. The mass flux through the former is only one thousandth that through the latter. Needless to say, however, this idea of Thevet's was picked up by other hydrographers and used as an explanation of other currents. That Thevet's explanation was a backward step as compared to the explanation of Peter Martyr is evident from the conservation of mass. If the Mississippi causes the Gulf Stream, then what becomes of the waters of the equatorial current?

The sharp line of demarcation between the warm and cold water masses was apparently first recorded by Lescarbot (1612):

> I have found something remarkable upon which a natural philosopher should meditate. On the 18th of June 1606, in latitude 45° at a distance of six times twenty leagues east of the Newfoundland Banks, we found ourselves in the midst of very warm water despite the fact that the air was cold. But on the 21st of June all of a sudden we were in so cold a fog that it seemed like January and the sea was extremely cold too.

In 1590 John White took a trip from Florida to Virginia and remarked that in order to stay within the Gulf Stream one had to stand far out to sea, because along the coast there was a countercurrent —("eddy currents setting to the south and southwest" [Kohl])—this being the first mention of countercurrents on the shoreward side of the Gulf Stream.

The seventeenth century saw the colonization of the Atlantic coast of North America, and the Gulf Stream was of course traversed countless times at all latitudes. A number of studies of ocean currents were published at this time, works of most varied quality. Varenius (1650) published a very comprehensive description of the surface currents then known, and Vossius (1663) emphasized the complete circulation of the North Atlantic as an ocean-wide clockwise motion. The first chart showing the Gulf Stream was Kircher's (1678), followed by a current chart by Happelius (1685). In addition to showing certain true features, these charts also show certain peculiar phenomena—for example, two distinct surface currents which cross over one another, and a great whirlpool off the Lofoten Islands, the legendary Maelstrom. On the whole, however, these charts were far superior to the theories advanced to explain them. Vossius contended that a great mountain of water was formed each day at the equator by the heat of the sun, and that this was carried westward and broke upon the American shore, and then flowed along the coasts in the form of currents. Kircher, it is true, suggested that the trade winds contributed to the ocean circulation, but he also enumerated other, more fantastic causes. Even well-informed men like Kepler had curious ideas about the causes of ocean currents. Kepler believed that because the water is only loosely attached to the earth it could not keep up with the diurnal rotation and hence fell behind—the result being the westward drift of the equatorial current.

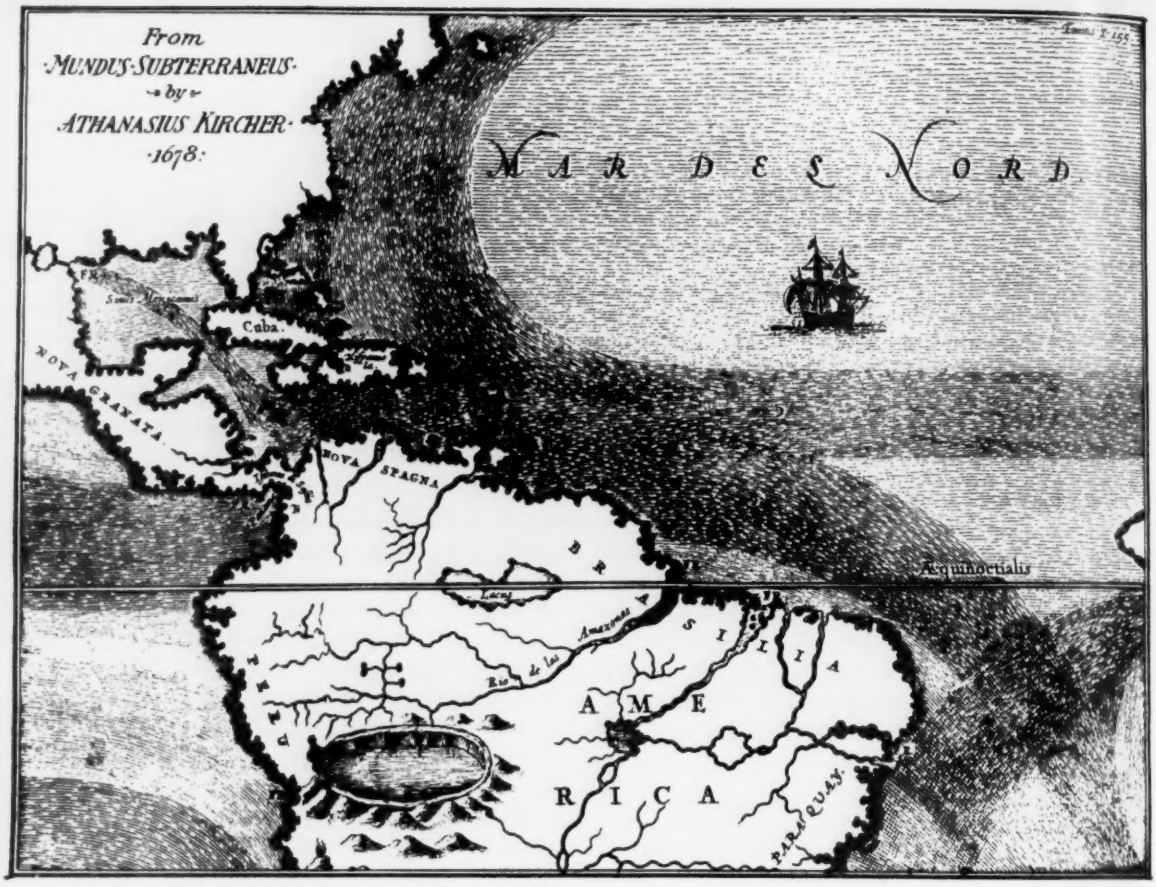
These various theories were at least honest attempts to explain physical phenomena by an as yet poorly developed physics. In addition there were advanced ridiculously fantastic theories that enjoyed a certain popularity. An example is Merula's statement (Kohl) that:

> At the North Pole one finds four large islands . . . between which are four deep and broad channels. The water flows together near the Pole, but at the Pole itself is a great Black Rock, 33 leagues in circumference. Ships which once enter one of these channels never return, not even with the most favourable winds, and next to the Black Rock all the water is engulfed into the bowels of the earth, whence it flows through springs and river sources once again into the light of day.

In 1702 Laval (1728) observed what he thought were variations in the strength of the Florida Current associated with the north component of wind and stated that he believed this to be common knowledge among sailors.

From the Eighteenth Century to the Middle of the Nineteenth

During the early 1700s the great American whale fishery sent ships all over the world, and the names of such patches of sand as Nantucket became known in every land touching the sea. The practical knowledge gained by these seafarers did not appear widely in journals, but was handed down by a system of apprenticeship and by word of mouth. At the same time the basic understanding of fluid mechanics was growing. Daniel Bernoulli's *Hydrodynamica* was published in 1738, and the theoretical aspects of oceanic tides were brought to a high point in the studies of Laplace. The influence of these developments in theoretical



mechanics, and the general intellectual atmosphere of this Age of Enlightenment, discouraged further extraphysical and purely imaginative theories of ocean currents. (Yet even today such works occasionally issue from the pens of the susceptible.)

In 1770 the Board of Customs at Boston complained to the Lords of the Treasury at London that the mail packets usually required two weeks longer to make the trip from England to New England than did the merchant ships. Benjamin Franklin was Postmaster General at the time and happened to discuss the matter with a Nantucket sea captain, Timothy Folger. The captain said he believed that matter to be true (Franklin, 1786):

> We are well acquainted with the stream because in our pursuit of whales, which keep to the sides of it but are not met within it, we run along the side and frequently cross it to change our side, and in crossing it have sometimes met and spoke with those packets who were in the middle of it and stemming it. We have informed them that they were stemming a current that was against them to the value of three miles an hour and advised them to cross it, but they were too wise to be councelled [sic] by simple American fishermen.

Franklin had Folger plot the course of the Gulf Stream for him and then had a chart engraved and printed by the General Post Office.

Franklin believed that the Gulf Stream was caused

> by the accumulation of water on the eastern coast of America between the tropics by the trade winds. It is known that a large piece of water 10 miles broad and generally only 3 feet deep has, by a strong wind, had its water driven to one side and sustained so as to become 6 feet deep while the windward side was laid dry. This may give some idea of the quantity heaped up by the American coast, and the reason of its running down in a strong current through the islands into the Gulf of Mexico and from thence proceeding along the coasts and banks of Newfoundland where it turns off towards and runs down through the Western Islands.

Franklin obviously did not have a thorough understanding of the effect of the wind stress on the sea; otherwise he would not have emphasized the shallowness of the small lake he offered as an example. Yet he did appreciate the action of the trades in producing the equatorial current and increasing the water level in the Gulf of Mexico. He says nothing of the prevailing westerlies. By the time of Maury (1850) Franklin's velocities of the stream were regarded as excessive, but more recent

studies (Iselin and Fuglister, 1948) tend to reaffirm them. Franklin does not show any details on the edge of the stream. Starting in 1775, Franklin, and Charles Blagden independently (1782), conceived the idea of using the thermometer as an instrument of navigation, and made series of surface temperature measurements while crossing the Atlantic. On Franklin's last voyage in 1785 he even attempted to measure subsurface temperatures to a depth of about 100 feet—first with a bottle and later with a cask fitted with valves at each end.

Following Franklin, a number of investigators made use of the surface thermometer, namely, Pownall, Williams, and Strickland (1802), the latter discovering in this fashion the northeasterly extension of the Gulf Stream toward England and Scandinavia. These temperature measurements were not made with any idea of determining the pressure field and geostrophic current, as is done today, but were simply regarded as an indication of the type of water through which the ship was sailing.

In passing, the invention and perfection of the marine chronometer by John Harrison and Thomas Earnshaw, respectively, should be mentioned. By 1785 accurate chronometers were generally available to ships, so that the determination of longitude at sea was at last a possibility, and the determination of the set of a current much more exact. Another important oceanographic tool, the drift bottle, was first used in 1802—the first having been cast from the *Rainbow* in that year. The use of drift bottles continued for more than a century, and finally received a great impetus at the hands of the Prince of Monaco.

One of the consequences of these early thermometric measurements was the noteworthy cruise of the packet *Eliza* en route from Halifax to England in April 1810 (Rennell, 1832). In the middle of the warm water of the Gulf Stream a mass of cold water with a temperature some 10°–15° colder than the surrounding water, and some 200 miles in diameter, was discovered. It was suggested that these cold spots were due to the melting of icebergs entrapped by the Gulf Stream. Although it can only be surmised, this occurrence suggests a phenomenon very much like the large cold eddy described lately by Iselin and Fuglister (1948). Another view, much in accord with modern synoptic experience, was that of John Hamilton, who made serial air and water temperature measurements during twenty-six voyages to and from Europe, and claimed that the Gulf Stream was so unsteady and shifted its position so frequently that it was impossible to define its limits. Both Humboldt (1814) and Sabine (1825) were convinced that changes in the strength of the trades affected the Gulf Stream, and the latter even suggested the use of weather ships which should observe the Florida Current and then sail quickly to Europe with the news of how strongly it was flowing, so that weather predictions could be made.

In 1832 the results of an extensive compilation by James Rennell of data from the British Admiralty Office was published posthumously. Rennell distinguished quite clearly between drift currents, which are produced by the direct stress of the wind, and what he called stream currents, which are produced by a horizontal pressure gradient in the direction of flow. Rennell regarded the Gulf Stream as a current of the second kind in accord with Franklin's earlier view and decided from his investigation that (1) the breadth of the stream changes from time to time; (2) the breadth can vary as much as twofold even within such a short time as ten weeks; (3) the variations are not seasonal; (4) the north side of the stream is more permanent than the south side; (5) temperature alone does not prove the existence of the stream, for even countercurrents may exist at the edge which are warm; and (6) cold water inclusions occur within the body of the warm water. Rennell proposed a special nomenclature for various portions of the Gulf Stream system.

The work of Rennell seemed to be so authoritative and exhaustive at the time that there was little doubt that the major features of the Gulf Stream had been delineated and charted. Few seriously entertained doubts that the Gulf Stream was a result of the downhill flow of water piled up by the trade winds along the American coast and in the Gulf of Mexico.

At this point a dramatic discovery was made which for a time completely discredited the notion that the winds are the cause of the ocean surface currents. Arago (1836) drew attention in 1836 to the results of a leveling survey across the Panama Isthmus by Lloyd and Ealmart which showed that the Gulf of Mexico was 3–5 feet lower than the Pacific. A line of levels across Florida showed a difference of not more than 7.5 inches, and probably less. That these levelings were subsequently corrected does not affect the historical development of the subject. Arago advanced the idea that the cause of the ocean currents is simply the density differences at the equator and poles due to unequal solar heating. "We should use the same theory for ocean currents which we use to explain the Trade Winds." Seafarers were still inclined to the wind-driven theory.

Last Half of the Nineteenth Century

In 1844 an intensive study of ocean circulation was begun by Matthew Fontaine Maury (1859), who was partial to the thermal theory. He dismisses the wind-driven theory of Rennell on the following grounds: (1) from a knowledge of the width of the current off Cape Hatteras he deduces the depth at Hatteras—completely ignoring the possibility of mixing of the stream with its environment; (2) the depth deduced in this manner is less than at the Straits of Florida; hence Maury makes the naïve claim that the Gulf Stream must run uphill and thus the wind theory is reduced to absurdity. He did not realize that the horizontal pressure gradient along the axis of the stream is not controlled by the slope of the bottom boundary, so that if the top surface slopes down toward the north, the current can perfectly well flow northward despite its diminishing depth. I cannot suppress the view that Maury was very much confused concerning fluid mechanics. (As a contrast it may be noted that Stokes' famous paper on the flow of viscous fluids was published in 1845 and Coriolis' famous work in 1835.) Maury uses examples such as the accumulation of sawdust in the center of a basin filled with water to explain the accumulation of sargassum in the Sargasso Sea, and the fact (?) that railroad trains usually run off the rails to the right as an example of Coriolis force—examples whose applicability to the ocean is extremely doubtful. He goes on to ask why, if the Atlantic circulation is a complete closed circuit, should there be a piling up at any one place. This shows that Maury did not understand Rennell's distinction between drift- and stream-currents. There seems to be little point in enumerating Maury's misconceptions about the effect of the earth's rotation on ocean currents, his roof-shaped current theory, his strange idea that waters of different salinities are immiscible. Maury says that the difference of density at the poles and the equator must produce then ocean currents, but he had no concrete idea about how salinity and temperature are related to density. Accurate hydrographic

tables had not yet been constructed, and the salinity distribution in the sea was only imperfectly known.

The encouragement which Maury gave to the collection of ship's data, his calling of the Maritime Conference at Brussels in 1853, and his dissemination of hydrographic information in the form of good pilot charts and revised sailing directions, however, were of great practical and commercial importance.

Modern survey of the Gulf Stream began with the work done by the U. S. Coast and Geodetic Survey under the direction of Franklin's great-grandson, A. D. Bache, starting in 1845. During the second year of the survey a hurricane overtook the tiny brig *Washington* and swept overboard ten men and the commanding officer, G. M. Bache, who first gave the name "Cold Wall" to the inner edge of the Gulf Stream. These early survey cruises confirmed the existence of cold veins within the stream. A. D. Bache supposed that the cold veins were a result of the bottom configuration which diverted the Gulf Stream in separate bands—although today such irregularities are not thought of as straight bands, but as eddies, and are attributed somewhat loosely to large-scale horizontal turbulence. At the time, however, the bands of cold water were supposed to be invariable in number and position, and the USCGS Gulf Stream Chart of 1860 shows them so. The currents were observed only at the surface by drift of the ship.

During the Civil War there was a lull in Gulf Stream investigations, but by 1867 they were resumed by Henry Mitchell, who attempted to measure subsurface currents by means of two floats, one of which remained at the surface, while the other sank slowly, the two being attached by a light wire. Mitchell concluded that the velocity of the Gulf Stream off Fort Chorrera, Cuba, extended undiminished to at least 600 fathoms. For the next ten years the *Bibb, Bache,* and *Blake* continued to survey the stream. Early soundings had been made by rope, but in 1881 Bartlett made use of the Thomson sounding machine, which employed piano wire. It is interesting to note that Bartlett did not detect the cold and warm bands shown on Bache's chart. According to the modern view of the temporary nature of cold inclusions in the Gulf Stream, this is not surprising. The *Challenger,* on her world-wide cruise, visited the Gulf Stream in 1873.

John Elliott Pillsbury (1891) commanded the *Blake* in a remarkable series of observations beginning in 1885, the ship being anchored at several sections in the Florida Straits and the current vector and temperature determined at various depths. The anchoring gear and the current meter were of Pillsbury's own design. As an example of the careful and painstaking manner in which Pillsbury went about his task, it may be noted that he took two years to occupy the section from Fowey Rocks to Gun Cay—"the time actually employed in observations at Section A being over 1,100 hours." Sections were also observed at four other locations across the Florida Straits, as well as one off Cape Hatteras and a number of stations in the passages of the Windward Islands. These investigations of Pillsbury stand out during the entire history of exploration of the Gulf Stream as examples of what a conscientious oceanographic survey should be.

Pillsbury confirmed the westward intensification of streamlines within the straits themselves, as earlier reported by Fremont. He established that Bache's hypothesis about mountain ranges under the sea causing the supposed bands of cold water was without a basis by sounding across the Gulf Stream all the way from the strait to Cape Hatteras and showing that the region was comparatively smooth. His observations of temperature and current in the Florida Straits are so unique that they are still used (Wuest, 1924) as the classical example of the accuracy of the geostrophic current determination. He was a firm believer in the wind-driven theory of ocean currents. He felt that the time variations of temperature horizontally and vertically along the Gulf Stream were abundantly proved, and attributed them to random local winds and tidal influences along the coast. He established the Antilles Current as a tributary to the Gulf Stream. He emphasized the importance of the prevailing westerlies in maintaining the north Atlantic drift. He surmised that the stream became increasingly meandering as it flows northeastward.

During the 1870s there was a good deal of discussion on a qualitative plane between wind-theory adherents and antagonists. The reader can get a pretty good idea of the level on which these discussions were carried by reading the opposing views of Carpenter (1874) and Wyville-Thomson (1874). A mathematical physicist, Zoeppritz (1878), attempted to show that the wind stress could cause appreciable ocean currents only after hundreds of thousands of years of constantly acting upon the water, because the molecular viscosity of water is so small. The importance of the role of turbulence in the ocean as an agency in the transfer of momentum, as well as of other properties, was not at the time appreciated. Reynolds' (1883) famous paper on his experiments on turbulent flow in pipes appeared five years later. The

gradual awakening to the fact that the ocean is essentially a turbulent regime showed the error in Zoeppritz' reasoning.

Reign of the Geostrophic Relationship

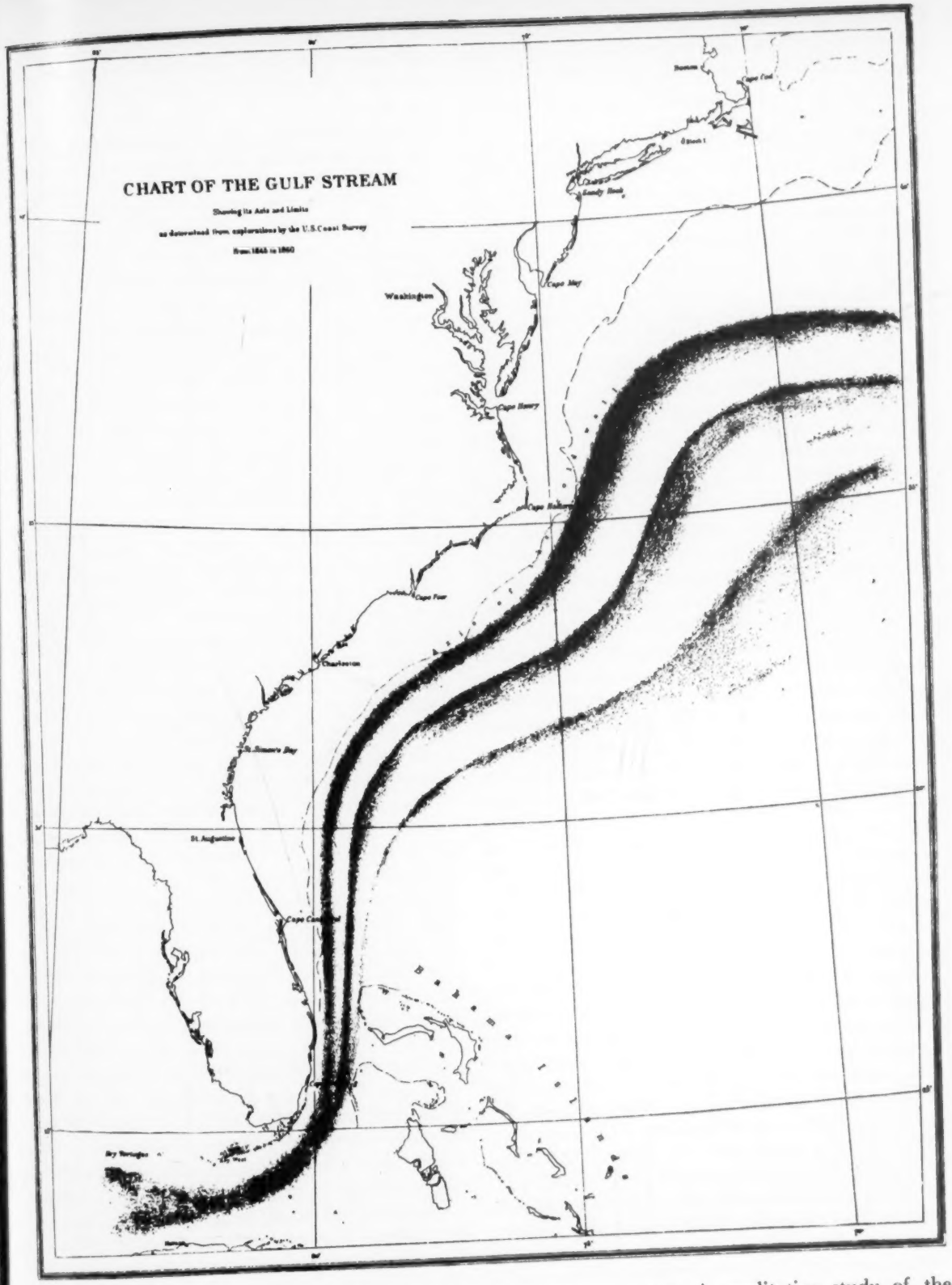
It is important to realize that throughout the entire period mentioned up until this point the influence of the earth's rotation upon ocean currents was only imperfectly understood by oceanographers, although the hydrodynamical equations of a perfect barotropic fluid relative to a rotating sphere had been written down by Laplace in 1775. The extremely important fact that the Coriolis force due to the main horizontal currents in the ocean is almost everywhere balanced by the horizontal pressure gradients due to the distribution of mass was not realized, despite the fact that these terms are several orders of magnitude greater than the driving and dissipative force terms. William Ferrel (1856, reprinted 1882) was one of the first to understand the role of the Coriolis force in the distribution of ocean currents caused by the wind. In a later paper (1874, reprinted 1882) he derived the relation between the barometric gradient and the velocity of the wind (which is the counterpart of the geostrophic current relationship of oceanography). For some reason these remarkable achievements of Ferrel were ignored by oceanographers, so that his work did not have a direct influence on the development of physical oceanography. Colding (1871) claims that as a result of the earth's rotation there must be a slope of the free surface up toward the east across the Gulf Stream to provide a horizontal pressure gradient sufficient to counterbalance the Coriolis deflecting force acting upon each moving water element. But Colding appears to have the mistaken idea that the Coriolis force acts only in an east-west direction. The formula for computing ocean currents from the slope of isobaric surfaces was derived by H. Mohn (1885).

The first careful study of the possibility of obtaining an approximation to the velocity field in the ocean from an exact knowledge of the mass field was due to Sandström and Helland-Hansen (1903), on the basis of V. Bjerknes' (1898) circulation theorem. It became evident that the Coriolis force acting upon the Gulf Stream was counterbalanced by a horizontal pressure gradient due to the mass of lighter water in the Sargasso Sea, and that the stream is not so much a warm current, but a boundary phenomenon associated with, but not caused by, the sudden downward slope of the isotherms toward the center of the ocean. It was recognized that the differences of density across the stream had nothing to do with the driving of the stream, but simply were part of an equilibrium brought about indirectly by the stress of the wind. The great step forward that this method of dynamic computation—as it came to be called—represented was that at last oceanographers had begun to use the hydrodynamical equations of motion in their study of the sea, even if they did so only in a piecemeal way.

By the turn of the century the dependence of density of sea water on its temperature, salinity, and pressure had been determined, and the ocean-wide distribution of salinity charted and plotted, largely because of the pioneering work of Forchammer (1868)—a development which came surprisingly late in synoptic oceanography, especially when one considers how much earlier the ocean-wide surface temperature was known.

We now enter into the modern period of physical oceanography, in which the hydrodynamical equations of mean motion are used, although never in their complete form. The reasons for using only a few of the terms are: First, the equations are nonlinear in their complete form, so that mathematical methods for solving them are unavailable. Second, the parameters depending upon turbulence are very poorly known on account of the mystery surrounding turbulence even today. These difficulties in specifying the mutual dependence of turbulence and the mean field of flow are not peculiar to oceanography, but are the fundamental unsolved problem of all of fluid mechanics. Our ignorance of the physical nature of turbulence has hobbled our understanding of the Gulf Stream in recent years. The third difficulty under which we labor is that even today we still do not have as much accurate synoptic information about the Gulf Stream as we should like.

V. W. Ekman (1902) developed a wind-drift theory, including the effect of the earth's rotation, which for the first time made possible computations of the effect of wind stress in transporting water. The ocean was regarded as homogeneous and of infinite extent horizontally. A constant wind blows over this ocean. The current vector in deep water at the surface is 45° *cum sole* to the wind and changes direction and diminishes with depth according to the Ekman spiral. The vertical eddy viscosity and Coriolis parameter are essential parameters in the problem. Ekman devised various modifications of this solution, including such extra effect as that due to a horizontal pressure gradient, and Fredholm (Ekman, 1905) obtained a transient solution. In the years that followed, Ekman carried out further investigations of ocean currents (1923,



1932, 1939) in which he discovered certain deflecting effects due to inequalities in the depth of a homogeneous ocean. A qualitative study of the effect of bottom topography upon currents in a

heterogeneous ocean has been advanced by Sverdrup (1942). Certain of these deflections have been apparently observed (Neumann, 1940) in the northern portions of the Gulf Stream system associated with the Altair cone.

LeDanois (1934) advanced a descriptive theory of certain periodical changes in the northern limits of the Gulf Stream system under the name of the "transgression theory." It appeared at a time when the influence of meteorological thought was strongest in oceanography, and did not lead to further developments.

The Past Fifteen Years

One of the primary objectives of the Woods Hole Oceanographic Institution at the time of its founding in 1930 was to investigate the Gulf Stream. An exhaustive account of the results of the first several years of exploration of the *Atlantis* is given by Iselin (1936) in his study of the circulation of the North Atlantic on the basis of temperature and salinity sections. The importance of lateral mixing is emphasized, and portions of various water masses through which the Gulf Stream passes are shown to be entrained and mixed with the main current. An increase of mass transport of the stream after it has moved north of the straits is set forth. This suggests that in some sense the existence of the Gulf Stream might be independent of the existence of the straits and that there must be some general cause acting which presses the streamlines of the general horizontal circulation toward the western edge of the North Atlantic Ocean. Iselin points out that there was no explanation at the time for this narrowing and intensifying of the stream along the American coast. A further very important point is that the temperature-salinity correlation is similar on both sides of the stream, although the temperature-depth and salinity-depth curves are very different.

Rossby (1936) proposed that many of the features of the Gulf Stream might be explicable by analogy with the spreading of a turbulent wake stream as previously studied for engineering applications. One of the contributions of this paper was Rossby's emphasis upon lateral stresses. Rossby deduces by heuristic reasoning that there must be a countercurrent between the coast and the stream as a result of the entrainment, and that this countercurrent is constrained to reach its maximum close to the western boundary of the main current. On the eastern side of the Gulf Stream, where there is no coast, Rossby does not obtain a countercurrent, but instead a rather broadened and extended mass of water moving with the Gulf Stream on account of the horizontal diffusion of momentum outward. The Gulf Stream is therefore not to be regarded as a river in Maury's sense, but as a

> terrestrial wake stream in a stratified medium [which] acts like a series of sinks with respect to the surrounding medium. Large quantities of fluid must be removed from the surroundings and this removal is associated with the creation of a countercurrent of strong anticyclonic vorticity along the left edge. The mass transport increases downstream but is intermittently restored to a prescribed value through the discharge of eddies along the left boundary of the current.

As Rossby himself makes clear, the reasoning leading to these results is not entirely based upon the equations of motion, but rests in part upon intuition, in part upon the actual observed facts about the Gulf Stream. The wake stream explanation of the countercurrent is still the only one to this day, probably because this theory is the only one which has included the nonlinear inertial terms of the equations of motion. The linear wind-driven theories of ocean circulation (Stommel, 1948; Munk, 1949) do not predict the observed countercurrent, and a numerical check shows that it is here that the nonlinear terms are not negligible, although they appear to be negligible elsewhere in the oceans. An interesting experimental study of a jet stream in a rotating tank was performed by Spilhaus (1937), which bears out many of the main points of Rossby's theory.

Rossby also investigated other phenomena associated with inertia currents (1937–38) in a study of the mutual adjustment of the velocity and pressure fields in a simple impulsively started straight parallel stream in a homogeneous ocean with lateral viscosity. The results seem to indicate that under these conditions (far removed from a coast) weak countercurrents are formed on both sides of the current, due to the lateral diffusion of momentum and associated Coriolis effects. Rossby (1938) also showed that long surface waves were capable of carrying away a considerable portion of the energy of an impulsively started stream during the period of its adjustment to the pressure field. This treatment is refined by Cahn (1945). It is difficult to see how to apply the results of these papers to the real Gulf Stream, although they may provide a clue for the appearance of internal waves along the thermocline on both sides of the Gulf Stream.

The emphasis of the papers above is upon time fluctuations in the mass transport and position of theoretically simplified ocean currents. Much of the difficulty in discussing the possible time fluctuations in the position and transport of the Gulf Stream derives from the fact that the oceano-

graphic observations available are rather sparse and incomplete. Three more recent studies have been made of the observational material (Montgomery, 1938, 1941; Iselin, 1941).

Tide gauge records at two points on opposite sides of an ocean current, if unconnected by an accurate leveling survey, cannot show the difference of level across the stream, but should indicate changes of level and hence of the geostrophic current. Tide gauge records at two points on the same side of a stream, and connected by an accurate leveling survey, might be expected to show some indication of the hydraulic head driving a current, and even possibly the effects of time fluctuations in the geostrophic current, although such changes of level might be due to other causes, which might be local meteorological or long-period tides. From an analysis of tide gauge records at points on opposite sides of the Gulf Stream, Montgomery (1938) finds that there is evidence of annual variations of 16 per cent of the total transport of the Gulf Stream, but he warns that this result is very uncertain. In a later paper Montgomery (1941) shows that there is no justification for using the difference of sea level between Key West and Miami (from tide gauge and leveling measurements) to obtain changes in the hydraulic head between these two points, and hence changes in the strength of the current through the Florida Straits. He has advanced (1947) the ingenious idea of using a liquid-filled submarine tube across the Florida Straits to measure directly the difference in level across the Florida Current.

Iselin (1941) exhibits, in addition to the above tide gauge data, fifteen sections of temperature and salinity across the Gulf Stream from Montauk Point to Bermuda. The mass transport was computed across each of these sections by means of the geostrophic relationship, and fluctuations in the computed values are shown to exist. Iselin believes that these fluctuations demonstrate a real seasonal fluctuation in the mass transport of the Gulf Stream, although the range of fluctuation of the transports lies very nearly within the range of probable error of measurement. The results were intended to be only preliminary in nature. Originally it was planned to complete the studies with more observations, but World War II intervened.

> For the period June 1937 to January 1940 the available subsurface observations indicate that the transport of the Gulf Stream has varied between a maximum of about 93 and a minimum of about 76 million cubic meters per second, assuming the 2000 decibar level as motionless. The sections analyzed dynamically indicate that the current is relatively strong during the early summer, falls off rapidly in strength to a minimum in October or November and then increases rapidly until January or later. A secondary minimum is called for in April or May, but it is not clear from the available transport determinations whether or not the prolonged winter maximum usually surpasses the summer maximum.

As an explanation for this semiannual rhythm Iselin advances the following schematic picture:

> . . . the major, permanent, warm currents of the North Atlantic are comparable to a huge eddy, from which the shallower offshoots, such as the Norwegian Atlantic Current, are derived. The Gulf Stream System forms the western and northern quadrants of the circuit, while the Canaries Current and the Northern Equatorial Current constitute the eastern and the southern. Because of the seasonal variation in the strength of the anticyclonic wind system over this area and its annual north-south migration, the main circular currents will vary in transport. In winter, when the torque of the winds increases, the Gulf Stream can be expected to strengthen. But in summer, when the winds move northward, a larger percentage of the North Equatorial Current is able to supply the Gulf Stream without passing through the Caribbean and the Straits of Florida. Thus the friction centered in the southwest is lessened and the Gulf Stream increases in volume, although the summer winds are comparatively weak. This seems to explain the double annual rhythm called for by the tide gauge record and by the computed transport values.

An important fact that has been emphasized (Dietrich, 1937; Sverdrup, 1942) but not completely explained is the rise in sea level along the coast of North America as determined from precise levelings. Sverdrup has shown that a slope current of only 10 cm sec^{-1} to the south along the immediate coast would be consistent with the sea-level rise, but there does not seem to be any explanation of the reason for this slope current.

A survey of newer results in the study of the Gulf Stream is given by Iselin and Fuglister (1948). The introduction of the bathythermograph, as developed from the original design by Spilhaus during World War II, made it possible to obtain a nearly continuous temperature-depth picture from a ship under way, so that the observer has a clear idea of what is going on at the very instant of observation and so can maneuver the survey vessel to explore interesting features. Moreover, the new methods of obtaining a fix, such as Loran, increase the precision of drift measurements. Several recent cruises of the *Atlantis,* using these new methods, have established without doubt the meandering of the Gulf Stream north of Cape Hatteras; that is, that there is a variation in the latitude of the point of maximum velocity of as much as a mile or two a day. This compares more or less with Rennell's earlier surmises. The maximum velocity of the stream is found to be of the order of 4 or 5 knots, and even the turbulent eddies

reach 3 knots. Transient horizontal eddies of many sizes are reported, ranging from a diameter of several miles (Spilhaus, 1940) to several hundred miles (Iselin and Fuglister, 1948). Iselin suggests that the smaller eddies are frictionally driven, whereas the larger eddies perhaps are similar to the large-scale developments in the strong westerlies of the upper atmosphere. Another possible view (Stommel, 1949) is that all the eddies are part of an eddy-cascade in the sense of Kolmogoroff (1941). The theory and observations of oceanic turbulence are too undeveloped as yet to test these views, however.

H. Panofsky (1949) has made a theoretical study of the instability of horizontal waves on the edge of certain simple current systems which are in some respects analogous to the Gulf Stream. It appears that perturbations of the stream decay where the stream is near to the continental slope, but are unstable, and could conceivably grow into the observed meanders, where the stream is at some distance from the slope. The period of the waves is two weeks, and the amplitude triples in two days. These predictions appear to be qualitatively acceptable, at least, but the proof requires further observation.

The observation of this middle range of oceanographic phenomena (of the order of size of several hundred miles) seems likely to be particularly difficult. The largest-scale distribution of properties, such as the main features of the general horizontal ocean circulation, change so slowly with time that monthly mean charts and data assembled from many separate years give a reasonably coherent picture. Small-scale phenomena (of the order of size of less than a mile) may be observed from a single ship, even though they change their features rapidly. Those phenomena of middle range of scale are, however, large enough to require that the observing ship travel through them, and since this traveling takes time their features may change sufficiently so that one ship cannot obtain a good instantaneous picture of them.

In an attempt to explain the westward intensification of streamlines in the North Atlantic circulation, which is the Gulf Stream, Stommel (1948) studied theoretically the wind-driven circulation of homogeneous water in a rectangular ocean. He found a concentration toward the west occurred if the variation of the Coriolis force with latitude was taken into account. There were several rather arbitrary simplifications in the theory, in particular the form of the friction terms. This study suggested, as indeed was implied in the schematic sketch made earlier by Iselin (1941), that the Gulf Stream would be much the same even if the Straits of Florida were dammed up. An obvious example of such a case is the Kuroshio Current off Japan. The theory of wind-driven currents in other sectors of the ocean was considerably developed by Sverdrup (1947) and Reid (1948). The most recent and complete study of wind-driven ocean circulation is due to Munk (1949), who has combined many of the features of the work of Rossby (1936), Sverdrup (1947), and Stommel (1948) in a thorough investigation of the steady state. Munk uses lateral eddy viscosity instead of Stommel's less realistic damping term; from Sverdrup he uses the vertically integrated horizontal equations of motion, thus avoiding the restriction of homogeneity; he uses the actual wind stress distribution; he retains the important planetary vorticity term, and obtains entirely on the basis of deduction the *correct* order of magnitude (65×10^6 m^3 sec^{-1}) of the transport of the Gulf Stream. Thus it appears from Munk that the wind stress acting upon the water is a sufficient cause to produce the major features of the North Atlantic circulation.

The nature of Munk's solution, although more satisfying and based upon measured physical constants, is broadly similar to Iselin's sketch (1941) and Stommel's model (1948), but it contains an additional interesting feature: a countercurrent in the vicinity of Bermuda, a feature quite independent of the form of the zonal wind distribution.

This is the state of affairs at present. It is believed that not only the Gulf Stream, but the entire horizontal circulation of the world's oceans, may be attributed *quantitatively* to the wind stress. A quantitative study of the thermohaline circulation has not been made, however, primarily because of its essentially nonlinear character. Therefore, there is still a possibility that in the deep ocean, and locally, the thermohaline circulation may dominate.

And, finally, one should note that only the steady-state wind-driven circulation has been solved. A satisfying theoretical account of the transient state is yet to come.

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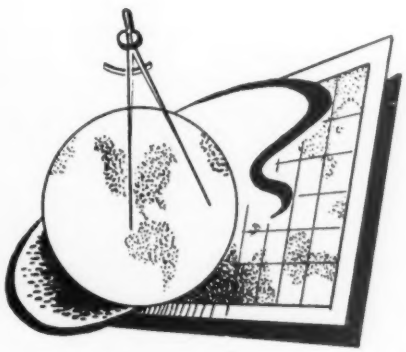

From the original in the Bodleian, Library, Oxford.

Dr. William Gilbert of Colchester

RUFUS SUTER

The author (Ph.D., Harvard, 1932), formerly a Fellow in the Asiatic Division of the Library of Congress, and now with the Army Map Service, Washington, D. C., gathered the material for his article while on a trip to Colchester last summer. He is a frequent contributor to THE SCIENTIFIC MONTHLY.

"Gilbert shall live till loadstones cease to draw,
Or British fleets the boundless ocean 'awe."
—Dryden, *Epistle to Dr. Charlton.*

THIS verse, like the next-to-the-last sentence in the memorial tablet on the wall of Holy Trinity Church in Colchester, suggests that among his contemporaries Gilbert's importance was thought to consist in his practical contributions to navigation. This penultimate sentence reads in translation: "He composed a book celebrated amongst foreigners, concerning the magnet, for nautical science." Edward Wright's dedicatory epistle standing at the head of the "book" gives the same impression. Possibly Wright wished to sell it to a sea-minded public, who were relatively as numerous in that age as an air-minded public is today, and so he emphasized the parts dealing with a proposed way to determine longitude and latitude at sea by magnetized needles.

The full title of this treatise is *Guilielmi Gilberti Colcestrensis, Medici Londinensis, De Magnete, Magneticisque corporibus, et de magno magnete tellure; Physiologia nova, plurimis & argumentis, & experimentis demonstrata.* It was printed in London in 1600 by Peter Short, whose shop was on Bread Street near Wingfield House, in the vicinity now laid low by wartime bombings. There is a prophetic significance in the date. In this same year

Giordano Bruno was burned in Rome, symbolizing, as Professor A. N. Whitehead once said, the death of the old speculative philosophy. The publication of *De Magnete* may be said to symbolize the birth of the new experimental science.

To understand the contemporary interpretation of Gilbert's importance we must recall that in his age the maritime nations were interested in finding a way to determine longitude at sea. The Spanish government offered a prize of 1,000 crowns for such a discovery in 1598. One of Galileo's pet projects, almost forgotten today, was to sell a plan he had devised. From about 1612 until his death thirty years later he negotiated unsuccessfully with the governments of Spain, Tuscany, and Holland. His scheme was to use Jupiter's moons, which he had discovered. For a clock he proposed the pendulum, the isochronism of which he had also discovered. For a telescope he suggested a helmet fitted out with telescopic eyepieces; and for a place for the navigator to sit, where his observing would not be disturbed by the ship's motion, he recommended a floating stool in a tub of water or oil. If accurate ephemerides were calculated for the configurations of Jupiter's moons as seen from Florence (and he calculated them), the navigator would have to take only the difference between the times predicted in the ephemeris and the times the configurations were observed from the ship at sea and multiply by fifteen to get the longitude as measured from Florence.

A method with more popular appeal, and at first glance more practical than Galileo's, was given to the public unintentionally by Columbus. In his records he states that in his second voyage (1496) he used for steering the observations on the declination of the compass needle that he made during his first voyage. Once this information was bruited abroad the age-old weakness for wishful thinking led people to jump at conclusions. No doubt the declination of the needle varies systematically with the longitude! Or, no doubt there are magnetic longitudes, and they may be converted into geographical longitudes simply by consultation of a table! Sebastian Cabot's "secret," which on his deathbed he declared to be a divine revelation, was probably this idea. Gilbert in *De Magnete* cites many who were hypnotized by the tantalizing promise, including the great Dutch physicist Simon Stevinus. Even Wright, the year before the appearance of *De Magnete* with his introductory epistle, published a work of his own, *The Havenfinding Art* (an adaptation of a book by the Dutchman just mentioned). Wright in his introduction to *De Magnete,* however, admitted that the method was not yet successful and asserted only that it pointed in a possibly fruitful direction.

I

Gilbert's role in this matter is only partly to his credit. In mature, scientific fashion he analyzed the data on declination (or "variation," as he called it, reserving the term "declination" for "dip"). He was able to accumulate considerable data from the legion of returned explorers of the seas. His conclusion was that no systematic relation holds between the declination of the compass needle and the longitude, so that in so far the hopes of the mariners were abortive. He drew two inductive inferences, however, that were not so fortunate. One was that the presence of large land masses is the cause of the declination of the compass needle, or, in modern terminology, that the isogonic lines conform to the shore lines of the continents. The other was that at a given point on the earth's surface the declination of the compass needle remains forever the same; in other words, that the isogonic lines are fixed. As a matter of fact, the magnetic longitude changes from age to age, possibly in cycles of about five hundred years; but because one cycle has not yet been completed since the discovery of the declination of the needle the period has not been ascertained.

Gilbert thus felt justified in stating as his final considered judgment that the method of finding the port on long voyages by accurate knowledge of the variation is of great importance. What misled him were his two inductive inferences, although he reached them by a method theoretically correct. That they were false was the fault of the inadequacy of the data available in that age, but, whatever the extenuating circumstances, his verdict served to bolster up for a time confidence in a method of determining longitude at sea that in reality was worthless. He was aware, however, that no instruments for measuring the amount of the declination of the compass needle had yet been invented that could be used on the rolling deck of a ship.

The outcome of this search for a way to find the longitude at sea is interesting. When the British Parliament came to the point of offering a prize, it did so in grand style: 10,000 pounds for a method accurate to 60 miles; 15,000 pounds for one accurate to 40 miles; 20,000 pounds for one accurate to 30 miles. The solution, when it came, was not offered by a pure scientist or philosopher, but by a technician, John Harrison of Faulby, Yorkshire, who invented the ship's chronometer.

Harrison received the full amount of the award two years before the American Revolution.

The problem of how to find the latitude at sea was already solved in Gilbert's day. North of the equator and on clear nights it was only necessary to observe the altitude of the Pole Star, which gives the northerly latitude directly. Indeed, with the aid of tables the latitude, whether north or south of the equator, could be determined by measuring the altitude of any star or the sun, but only at the instant of culmination. When the sky was cloudy, of course, this method failed. Then the mariner floated blind on a literally trackless sea. Gilbert thought he had found a gadget by which the latitude could be determined in the cozy interior of the ship's cabin even when the fiercest tropical storm raged outside. This instrument, an improvement on Robert Norman's dipping needle, was the same sort of magnetized needle as in a compass, but mounted so that it would swing perpendicularly rather than horizontally. Thanks to a few data brought home by explorers and to many observations of his own on lodestones, Gilbert was convinced that the dip, or, as he sometimes calls it, the "declination," of the compass needle is constant for any given latitude. In other words, there are magnetic parallels which, in spite of small variations, bear a fixed and systematic proportion to the geographical parallels. The latitude, thus, could be calculated by checking the readings of the dipping needle against the curves of an intricate graph. Wright in his preface to *De Magnete* was certain beyond any shadow of doubt that this scheme would work.

He was not the only one of the doctor's group at Wingfield House to be spellbound by this proposal. There were also Henry Briggs and Thomas Blundeville, the mathematician and the free-lance writer. All four men collaborated on a mariner's handbook in which the dipping needle scheme for determining latitude was developed. Published two years after *De Magnete,* under Blundeville's name, it had the longest title on record. It contains two hundred nineteen words, launching off with: *The Theoriques of the Seven Planets.* . . . In this work two instruments attributed to Gilbert are described: a dipping needle and a latitude gadget by which the readings of the dipping needle could be converted into latitudes. Gilbert feared, however, that the rolling of the ship would make the use of this latter device difficult. Hence an extra appendix was added which contained a note by Wright introducing a mathematical table calculated by Professor Briggs to serve as a more practical alternative to the latitude instrument.

One wishes that all Gilbert had hoped of the magnetized needle had come true. But its ability to tell the latitude, like its power to indicate the longitude, proved disappointing. Again inadequacy of available data rather than error of method was the reason for the failure. Among the misleading inferences that vitiated the scheme was again the mistake that magnetic readings for a given point on the earth's surface stay forever the same; also, the erroneous opinion that the magnetic and geographic poles of the earth coincide. As a matter of fact, the magnetic poles at the present time are about 1,400 miles away from the geographical poles. In these unfortunate excursions into practical navigation the most that can be said for Gilbert is that he had a prevision of what we now call instrumental control.

Regardless of the contemporary estimate of Gilbert's importance, time has proved that his practical contributions to seamen were nil. His value so far as nautical science is concerned lay in a different direction. That was in a master stroke of genius by which he uncovered one of the elemental physical facts of the earth. For Gilbert was the first human being to recognize that the earth is a magnet, an insight certainly as remarkable as the inference that the earth is an oblate spheroid, or that it spins, or that it swings round the sun. The importance of this knowledge for nautical science was not so much practical as theoretical. It provided the seaman, if he was interested, with an explanation of why the compass points to the approximate north. The action of the needle was a special case under the rule that opposite poles of magnets attract. Gilbert by his inference that the earth is a magnet created the science of terrestrial magnetism; for without his discovery the science would have had no object.

His procedure in making this epoch-making contribution to pure science is a brilliant example of ingenuity in devising a specialized technique for dealing with a problem for which there was no methodological precedent. The instrument he used was a globular lodestone, or *terrella* ("earthlet"). By observing the directions magnetized iron wires took when spread about on its surface, and by studying the action of pivoted needles placed in its vicinity, he was able to mark the north and south poles, the equator, the parallels of latitude, and the meridians of longitude. With acid he burnt out holes to create the differences in elevations between continents and seabeds. This was the technique that gave birth to the science of terrestrial magnetism. By it the earth was reduced to a size suitable for laboratory study. The macrocosm was too large to submit to manipulation; but the microcosm

could be handled like a guinea pig under conditions permitting of controlled experiment and observation.

One wonders at once whether the idea that the earth is a magnet was Gilbert's initial methodological assumption or his final inductive conclusion. The answer is not clear, but in all probability both interpretations are true. At the beginning he was struck by random coincidences between *terra* and terrella. This is the usual way in which hypotheses are suggested to the appropriately tuned mind. But it was only after a number of coincidences had occurred that Gilbert began to use the equivalence as a guide; began to see something more than a coincidence in the fact that in this respect and that the earth behaved like a magnet. Sometimes he checked against the terrella the data furnished by explorers; sometimes vice versa. Frequently he just drew parallels between the two. In any case, as coincidences multiplied the initial assumption became more and more plausible.

One often feels, however, that the good doctor allowed his mind to be directed more by terrella than by terra. This was because genuine geographical data were hard to get and even when available were undependable. The data from the terrella, on the other hand, were as precise, accurate, and abundant as Gilbert cared to make them. This predilection for the terrella was a source of error in Gilbert's attempts to formulate specific laws of terrestrial magnetism, for, after all, the earth is not merely a big terrella.

He fell into misunderstandings, to be sure; but on the whole his procedure was successful. Poring over his toy world he made some howlers, but no more than scientific theorists make today. The fact remains that the chief revelation of his technique was sound. His technique suggested to him first the hunch that the earth is a magnet; second, it provided a systematic plan for contributing progressively to the full verification of this hunch. Gilbert prosecuted the plan far enough to remove any reasonable doubt that his hypothesis was correct.

II

An erudite citizen of Colchester named Charles E. Benham published a short and brilliant biography of Gilbert early in the present century, a year before the town boomed in a kind of "old home week" in honor of the tercentenary of its most illustrious son's death. As a brother Colchesterite, Benham felt that he was free to offer a few demurrers to the praises that were starting to pour in from all quarters. He wrote that it would have been better if the paragraphs on electricity in *De Magnete* had been expunged. His reason was forthright. There were too many mistakes, and what was true was trivial. On the other hand, an electrical engineer, Silvanus Thompson, who had been an almost continuous biographer, translator, and commentator of Gilbert for more than a decade before the Colchester jubilee, recommended the paragraphs on electricity as the most intriguing part of the book. His beloved master was the father of electrical science, the original stimulus of the burst of intellectual energy that produced Faraday, Lord Kelvin, and the electrical age. Here we have an example of how radically biographers may differ in their interpretation of the same facts.

Gilbert's spadework in the science of static electricity barely broke the surface. Nonetheless, a surprisingly large part of the laboratory discipline in that part of the high-school physics course dealing with static electricity is a repetition of his experiments and observations. The impressive difference between Gilbert and the high-school pupil is that Gilbert had no teacher. He received from the earlier ages only the item that a yellowish fossilized vegetable resin called amber, and a black fossilized matter akin to coal named jet, when rubbed, attract straw and other light objects.

When today's young sophomore tests the mysterious attractive force of electrics with the electroscope, he uses an instrument invented by Gilbert. The original electroscope was a delicately balanced pivoted splinter called a nonmagnetic versorium. Many of the kinds of object he tests, moreover, were examined one by one four centuries earlier by Gilbert. The old doctor increased the list of electrics by many items, including glass, sealing wax, and sulphur, all of which were important in subsequent electrostatic experiments. Most of the twenty contributions attributed by Silvanus Thompson to Gilbert are rediscovered each year by myriads of eager high-school pupils: thus the observation that the lodestone, in spite of its capacity for magnetic attraction, fails to affect the splinter in the electroscope. This phenomenon, incidentally, was remarked upon about twelve centuries earlier by so unexpected a person as St. Augustine, who asks in the *City of God* why the lodestone which attracts iron should not move straws. Gilbert observed that electrics draw many other things besides straw. Their strange power affects metal, stone, earth, oil, water, and smoke if not too thin. He recognized the existence of a class of nonelectrics. He knew that electrics when roasted, burnt, or softened by heat, or when a live coal is brought in their neighborhood, do not attract. At the other extreme, damp weather hinders

electrification. Although magnetic force penetrates all substances save iron, electric attractions are selective to the point that a sheet of paper or a piece of linen, or even moist air blown from the mouth, screens them off. An electrified body, unlike a magnetized one, does not orient itself. Instead, the attraction of an electric is simply in a straight line toward itself.

Another contribution by Gilbert was the coining of a word that afterwards begot a whole vocabulary: "electric," "nonelectric," "electricity," and the rich crop of partial derivatives and compounds such as "electrolysis," "electrocution," "electrotherapeutics," etc. Since Gilbert wrote in Latin, it is not accurate to say that he invented the English word "electric." What he coined was the Latin term *electra,* which he aptly derived from the Greek ἠλέκτρον for amber, and defined in the plural as "Bodies that attract in the same way as amber." His expression for "electricity" was *vis electrica.*

Gilbert's failures of judgment in his electrical studies were as remarkable as his successes. They probably resulted from his lack of interest in the subject for its own sake. The whole narrative of his work in this field is a brief digression in *De Magnete,* where his aim is to emphasize the uniqueness of magnetism and its radical difference from other types of attractive force, including electricity. Unlike magnetism the *vis electrica* was for him a tyrannical power compelling the passive victim to collapse into union with the electric. Electrical attractions were one-sided, altogether distinct from the mutual, cooperative springing together of the lodestone and the magnetized iron. We know, since Newton, that such one-sided actions do not happen in physics. Another misjudgment was Gilbert's denial of electrical repulsion.

One wishes that he had carried his experiments with the electroscope a bit further and discovered what every schoolboy knows today, that there is a positive and a negative electricity, and that as with magnetism objects bearing unlike charges attract each other, whereas those bearing like charges are mutually repellent. But this would have brought electricity too close to magnetism. Gilbert's eyes were closed to the vista that led to the immensely fertile field of electromagnetism. He never caught a glimpse of the connection between electricity and lightning. He never dreamed of an electric current.

In one respect, however, his attitude toward electricity was more in tune with the subsequent pitch of the scientific chorus than were his off-key speculations about the cause of magnetism. His explanation of electricity was strictly mechanical. Three quarters of a century after *De Magnete,* another pioneer of modern science, Robert Boyle, gave to the world the first book about electricity, a tract of paramount common sense entitled *Experiments and Notes about the Mechanical Origine or Production of Electricity.* He refers affectionately to "our industrious countryman, Gilbert," and expounds his master's theory of the cause of electricity more understandably than Gilbert did himself. Says Boyle:

> The Amber, or other Electrick, being chaf'd or heated, is made to emit certain Rayes of Files of unctuous Streams, which, when they come to be a little cool'd by the external air, are somewhat condens'd, and having lost their former agitation, shrink back to the body whence they sallied out, and carry with them those light bodies, that their further ends happen to adhere to, at the time of their retraction: As when a drop of Oyl or Syrup hangs from the end of a small stick, if that be dextrously and cautiously struck, the viscous substance will, by that impulse, be stretch'd out, and presently retreating, will bring along with it the dust or other light bodies that chanc'd to stick to the remoter parts of it.

Gilbert's theory was also entertained by the French empiricist and opponent of Descartes, Pierre Gassendi, and by Sir Thomas Browne, author of *Religio Medici.* It set the fashion that dominated the scientific world for centuries that electricity is a fluid. (Even today we speak of electrical "current.")

For his studies of magnetism Gilbert used all the paraphernalia of the modern high-school physics laboratory: lodestones of various sizes and shapes, including the globular kind, or terrellae; artificial magnets, iron and steel filings, wires, bars, metal sheets, tiny wooden boats, a tub of water or oil, acid, a furnace, an anvil, a hammer, a file, chalk, compasses, dipping needles, versoria, or delicately poised pivoted needles, and a primitive torsion balance. He used controlled experiment and observation coupled with straightforward induction. His technique is as simple and direct as any paradigm to be found in a conventional twentieth-century textbook of the logic of the sciences. The fruit of his studies is the whole classical doctrine of the magnet familiar to every high-school pupil today. The discoveries and empirical proofs are too numerous to mention. A few of the more interesting ones, however, are worth describing. Unlike poles of magnets attract each other; like poles repel. Pieces of iron that have been magnetized at the same pole of a lodestone repel each other at the magnetized ends, and the opposite ends are mutually hostile. If a lodestone is cut in half the original north and south poles remain the same, but two new poles are created at the severed ends,

a new negative and a new positive one. If the north pole of a magnetized body is brought near the south pole of a much stronger lodestone, the iron is seized and the two are held in close embrace, because the polarity of the magnetized body is reversed by the stronger lodestone. Iron loses its capacity to be drawn when red-hot, but when cooled its power returns. Bars can be magnetized by hammering them when they are laid north and south, especially when they are being cooled from red heat. Magnetic forces penetrate flame and thick slabs of every material except iron.

Several phenomena were most conveniently demonstrated with the terrella. Thus, when the appropriate pole of a versorium is touched to either pole of a terrella, the attraction is at its maximum strength. The attraction grows proportionally weaker as the needle is moved toward the equator. It is feeblest at the equator. However, at no point is it entirely absent. Lodestone, like the artificial magnet, is magnetic throughout. Another peculiarity most conveniently exhibited with the terrella is that if part is cut away, such as a segment containing the original north pole, this pole reappears in a new position nearer the original center of the sphere. The other elements in the "geographical" grid likewise shift in order to preserve their relative positions. Also, the power of the lodestone as a whole is weakened by the removal of the part. If the segment is replaced, the elements of the grid spring back into their original positions, and the original strength is restored.

Gilbert discovered that for uniform lodestones the strength and range of magnetism are proportional to the quantity and mass. This, according to Sir William Dampier in his *A History of Science,* was the first time mass was conceived as something separate from weight, an advance of the utmost importance in the development of physics. Possibly Gilbert's idea influenced Galileo and Kepler and through them Newton. Gilbert discovered that the power of a magnet is increased if it is armed with soft iron caps. He introduced the term *armatura,* with this technological meaning. The English translation, of course, is "armature." Although Galileo is commonly credited with this invention, Galileo himself attributed it to Gilbert through the mouth of Salviati, the learned specialist, in his *Dialogo dei due massimi sistemi del mondo.* Gilbert also was the first to recognize what we now call the magnetic field, the space round the magnet where its attraction is observable. He styled it the "Orb of Virtue" or the "Sphere of Influence."

A few of Gilbert's narrower inductive generalizations have been described. The widest his mind could stretch in this process of generalization was to classify the kinds of magnetic action. In this endeavor he perceived clearly the distinction between the attractive power of the magnet and what he called its verticity, that is, its polarity, or tendency to turn, direct, or align itself to conform reciprocally with the polarity of another magnet. He enlarged the first of his types of magnetic action, attraction, to include not only the attractions between ordinary magnets, but also between the earth and the objects within its Orb of Virtue, among them the moon. He ascribed the irregularities in the moon's surface and its keeping the same face turned toward the earth to their both being magnets. In short, he equated gravitation with magnetism. Under his second class of magnetic actions, verticity, he subsumed not only the mutual tendency of ordinary magnets to direct each other and the tendency of the compass needle to direct itself to the poles of the earth, but also the alleged tendency of the earth's axis to point in one direction in the universe and to stay put. His other classes of magnetic action were variation, declination, and revolution. With the first two of these we are familiar. What Gilbert called "variation," we now term "declination." What he named "declination," or "dip," we now call "inclination." Gilbert's fifth and last class was revolution, or circular movement. It is odd that he failed to distinguish between axial rotation and orbital revolution. Apparently he thought of the difference as irrelevant to the mechanics or etiology of circular movement. Under this fifth class he included the axial rotation of the earth, the other planets, and the stars, and instances of orbital revolution such as the moon's movement around the earth, and the movements of the Jovian satellites around Jupiter.

His inclusion of this fifth class has often been criticized as muddled. It should be mentioned, however, that a recent theory of P. M. S. Blackett, of the University of Manchester, gives one the hint that Gilbert may have been on the right track after all. According to Blackett's theory, all bodies of matter generate a magnetic field when they rotate. His equation indicates also that the poles of the magnetic field lie along the axis of rotation, although he has left unexplained why this is not so with the earth. If this new theory is sound, Gilbert's error lay not in his seeing a connection between magnetism and rotation, but in his reversing the causal relationship. Why Gilbert included rotation and revolution as a class of magnetic action is a mystery. Certainly he never saw a terrella spin automatically just because it was a terrella.

Besides the mass of inductive generalizations about the way in which magnetic bodies behave, Gilbert proposed an explanation of the cause of magnetism. We catch our first glimpse of this in his warning that the term "attraction" is inappropriate. The word "attraction," he thinks, implies force and tyrannical violence in the lodestone and passivity in the iron. But the fact is that magnetic bodies come together by their joint forces and mutual strength. He prefers the word "coition," which, incidentally, was borrowed by Bacon. Gilbert was often at pains to emphasize that none of the types of magnetic action were the effects of one-sided compulsion. This was as true of verticity, variation, or dip as of coition. Always the movement was the joint result of cooperating powers, of a mutual will to conform.

As he proceeded in his effort to explain the cause of magnetism, he lapsed into an ancient terminology. The coition of magnetic bodies takes place by formal efficiencies. This form is unique and inheres in the principal globes. It is their proper entity, their being. He calls it their primary, radical, and astral form. Each star, the sun, the moon, and the earth have form. Thus fragments of the moon and sun adjust themselves in accordance with lunar and solar laws to conform to the moon's and the sun's contour as a lodestone does to another lodestone or to the earth. Gilbert's purpose in availing himself of this ancient word "form" was to make clear that magnetism is immaterial; no efflux and influx of physical particles; no humor, vapor, or fluid, as is electricity. Instead it is analogous to what the recent school of vitalistic biologists name "entelechy." To call the cause of magnetic coition a form is a recondite way of saying that magnets have a soul. This is precisely what Gilbert meant, for his explanation of magnetic behavior was that like any other living body the lodestone has a soul. At this level of cogitation, the father of the experimental method in the British Isles became a Neoplatonic mystic and espoused the weird teaching of Giordano Bruno that the earth, the other planets, the sun, moon, and stars are great animals with souls.

Before we write off this inspiration of Gilbert as a legacy from the prescientific past, it would not be amiss to consider that we still are in the dark about how magnets can possibly act at a distance. If there is ever an excusable occasion for dropping off the empirical wagon into the gloriously intoxicated world of the Neoplatonists it might be at this point.

Before we bring this discussion of Gilbert's work on magnetism to a close it might be well to emphasize the historical originality of many of his contributions and of his method. Prior to *De Magnete* a mixture of nonsense, romancing, astrology, and folklore passed for knowledge on the subject: for instance, that a lodestone will lose its virtue if rubbed with garlic! There was before Gilbert, however, one writer worthy of his salt, and to whom Gilbert never gave adequate credit. This was Peter Peregrinus of Maricourt, pupil of Roger Bacon and anachronistic empiricist in the thirteenth century. His experiments are described in his letter to a soldier, *Epistola . . . ad Sygerum de Foucaucourt militem de magnete.* Peregrinus even used the terrella. It must not be forgotten, either, as Gilbert did not forget, that the verticity of the magnetic needle was put to practical use by the Chinese before the time of Marco Polo.

III

Gilbert's speculations about the cause and the universality of magnetism bring us to his astronomy. Besides his other distinctions, he has the honor of being the first Briton publicly to accept the system of Copernicus. His discipleship was prompt and courageous, for the great Polish founder of modern astronomy died only one year before Gilbert was born; and the brand-new world view must have seemed as outrageous to the majority of solid citizens of his day as Darwinism did two generations ago, or as the economic interpretation of history does in our own time.

Gilbert's championing of Copernicanism, however was only partial. This is seldom appreciated. There is no need to point out that the system of Copernicus has two chief points: first, that the earth and other planets travel round Old Sol; and, second, that the earth rotates on its axis. This second point seems so obvious today that it is hard to believe that anyone ever thought otherwise; so that we tend to identify the importance and originality of Copernicus with the first point, the heliocentric theory. Thus it is with a shock that we learn that Gilbert taught that the earth rotates, but rejected the theory that the earth moves round the sun. Or, rather, it is with two shocks, for we must be as much astonished that Gilbert failed to see the truth in the whole of Copernicus' idea as that he achieved anything remarkable in realizing that the earth spins.

Edward Wright no doubt anticipated a scandal when he commented in his preface to *De Magnete* that Gilbert's arguments for the earth's rotation did not conflict with Holy Writ. Though terra firma spins, she spins in the same place; so, in a sense, she does not move; and so the Scriptures are

not contradicted. This was, indeed, Gilbert's notion. The earth simply whirls, like a giant terrella.

But innocuous as this slim Copernicanism was it did not fail to cause grim headshakings and raised eyebrows even in England. Archdeacon William Barlowe, who championed Gilbert's magnetic philosophy, beat a hasty retreat whenever the question of the earth's rotation came up. A medical colleague and fellow-experimenter on magnets, Dr. Mark Ridley, had a similar reaction. The Oxford epigrammatist, John Qwen, disposed of Gilbert's idea that terra spins by accusing him, in a Latin couplet, of being drunk. In Italy, of course, the reaction was more violent and official. Where the heresy was defended in the sixth and last book of *De Magnete,* it was mutilated or torn out in all copies that could be found, in the interest of the public morals. An anecdote preserved by Benham is that Galileo came into possession of a copy of *De Magnete* because an Aristotelian who wanted to get the sixth book out of his house gave the whole thing to the already "hopeless" astronomer.

The teaching that caused so much rumpus is presented by Gilbert as the innocent offspring of common sense. It is more sensible to assume that the small earth turns from west to east than that the gigantic solid crystalline spheres, studded with all the heavenly bodies, rotate from east to west. Why more sensible? Because the spinning of the spheres would have to be too fast. Neither Gilbert nor anyone else in that age knew how far away the fixed stars really are; but he realized, at least, that they are more remote than the moon. When even the moon is too far away to be conveniently carried around the earth in the wall of a spinning sphere every twenty-four hours, it is insane to imagine that a sphere holding the fixed stars could complete a rotation in the same brief period. The argument was strengthened by Gilbert's realization that the fixed stars are at various distances, a discovery he was the first to make. It meant that every star would have to have its own crystalline sphere, and that these thousands of spheres would have to be synchronized to finish one spin at the same instant. The assumption that the earth rotates is of course much easier to swallow.

Another popular argument against the idea that terra rotates was that so large and heavy a body would break up if it whirled. Gilbert's rejoinder again depended upon common sense. It is more likely that such gigantic bodies as the crystalline spheres, with their speed of rotation, would split.

Gilbert's Copernicanism was not only ridiculed by the intelligent conservative public, scientific men, and ecclesiastics, but also by one of the most far-seeing of the prophets of our modern world, Sir Francis Bacon. The Lord Chancellor in a fascinating but astronomically inept tract, *Description of the Intellectual Globe,* reveals several facets of Gilbert's cosmology. He tells us that the good doctor held, along with several of the ancients, that the earth is a planet or star, and that all the planets and stars, together with dark invisible globes and shining spheres too distant to be seen, stud the recesses of space like islands in a vast sea. There is no common center around which they move, but all swing round their own centers, their axes. Some move, in addition, around centers outside themselves, like our moon. Now and then a center participates in a progressive motion. What is peculiarly informative in Bacon's account is the contrast he sets up between Gilbert and Copernicus. The latter, he says, was the first man to believe that the sun is the center, and that the earth, planets, and stars travel around it. He remarks that, foolish as the fancy of Gilbert was, it is preferable to the nonsense of Copernicus.

Bacon's source for these cosmological speculations of Gilbert was the manuscripts of Gilbert's work on the sublunary world, which he read possibly in his younger days. This book, edited by Gilbert's half-brother, William Gilbert of Melford, was published in 1651 by Elzevir in Amsterdam. It may have actually been written before *De Magnete.* Its full title is: *Guilielmi Gilberti Colcestrensis Medici Regii, De Mundo nostro Sublunari Philosophia Nova. Opus posthumum, ab Authoris fratre collectum pridem & dispositum, nunc ex duobus MSS. codicibus editum ex Museio viri perillustris Guilielmi Boswelli Equitis aurati etc. & Oratoris apud Foederatos Belgas Angli.* Some of the topics discussed are the elements, their proportions and composition; various phenomena such as heat, cold, natural attraction; the rotation of the earth; the nature, surface, and motion of the moon; the motion of the sun; the twinkling of the stars; comets, clouds, rain, wind, the rainbow, the sea, its saltiness, the tides, the Copernican hypothesis; that the sun, planets, and stars have souls; that the heavenly bodies are not stuck on the inner surfaces of crystalline spheres. The book has a map of the moon, with such terms as *mare, insula, sinus, promontorium,* etc. Gilbert must have had great faith in Queen Elizabeth's explorers, as one lunar *insula* he dubs "Britannia."

This book never caused the furor that *De Magnete* did, and no translation has ever appeared. Only an expert in Renaissance Latin also versed in the history of science would know enough to pass judgment on its historical worth.

SCIENCE ON THE MARCH

BIOLOGY OF FLOWERING IN PLANTS

THE flowering of some plants can be controlled by the use of environmental factors, such as photoperiod and temperature; other plants flower over a wide range of temperature and light conditions. To illustrate, soybean var. Biloxi (*Glycine Max*) flowers and fruits in an environment in which the daily number of dark hours is greater than the number of light hours, and it therefore is called a short-day plant. Stock (*Mathiola incana*) ordinarily blossoms where the daily number of dark hours is less than the light and is referred to as a long-day plant. Whether a species responds as a typical long- or short-day plant depends a great deal upon the temperature at which it is grown. A too-high or -low temperature may prevent a plant from flowering at either a long- or short-day photoperiod, or it may induce it to flower at both day lengths (Fig. 1).[1] Poinsettia ordinarily blossoms in short days; however, if the temperature is 75° F. or above at night, it fails to blossom in either short or long days. Tomato flowers regardless of day length and is therefore known as an indeterminate type.

Varieties of plants differ a great deal in the number of days of the proper photoperiod they require to form blossom buds. *Xanthium echinatum* requires one short day for the production of staminate blossoms, whereas two short days are necessary for the formation of pistillate flowers. At this time, the plants can be returned to a long-day environment and continue the development of blossom buds. Soybean var. Biloxi requires at least seventeen short days previous to its transfer to long days, for the development of blossoms and fruits. After twelve short days, blossom primordia are apparent, but their development is inhibited if the plants are grown under long-day conditions before the seventeen consecutive short days. Stock grown in the greenhouse are long-day plants and require at least twenty-four long days for blossom buds to develop sufficiently to mature into flowers. These results indicate that some plants must be exposed to an environment inductive to flowering for a greater length of time than just the induction period required for the formation of blossom primordia. It is not sufficient to secure only primordia, for unless the environment is favorable for their development into blossoms, abortion will occur.[2]

The differences in anatomical structure of stems of vegetative and flowering plants were reported in 1936.[3] As a result of this investigation, the anatomical structure of the stems and the development of the stem tip of the same plant were studied to determine the nature of the cellular structure from the time of induction until flowering. It was found that transformations in the anatomical structure were apparent soon after the beginning of a treatment inductive to flowering. *Salvia,* a short-day plant, may be used as an illustration. Stems of plants given five short days showed a less active cambium and a decrease in differentiation of vascular elements. At this time the stem tip had elongated, and conspicuous growing points in the axils of the leaves were apparent. Further reduction in cambial activity and increased development of the stem tip continued until flowering. The stems of plants that received thirty-nine short days were in flower, and by this time cambial activity had ceased, vascular elements were smaller, and there was an increased thickening of the cell walls (Figs. 2 and 3). Likewise for soybean var. Biloxi, a short-day plant, the differences in anatomical structure became more pronounced as the plants were given additional short days. Plants which were given thirty-nine short days, and which by this time had macroscopic flower buds, showed the absence of an active cambium in the stem. There was also an absence of meristematic tissue, and the cells had become increasingly thicker-walled. The alterations occurring in the stem and the stem tip in *Xanthium* and *Cosmos,* also short-day plants, were similar to those described for *Salvia* and soybean.

Stock flowers in long days, but the anatomical changes of the stem as flowering progresses are similar to those described for the previously mentioned short-day plants. Twenty-four to thirty days of long-day treatment resulted in the formation of blossom buds and a reduction in cambial activity and differentiation. It is apparent that alterations in the vascular structure occur relatively early in the development of plants exposed to an environment favorable to flowering. From the species examined it can be concluded that, although different environmental conditions must be used to induce flowering, a similar internal structural development of the stem occurs. It is evident that the

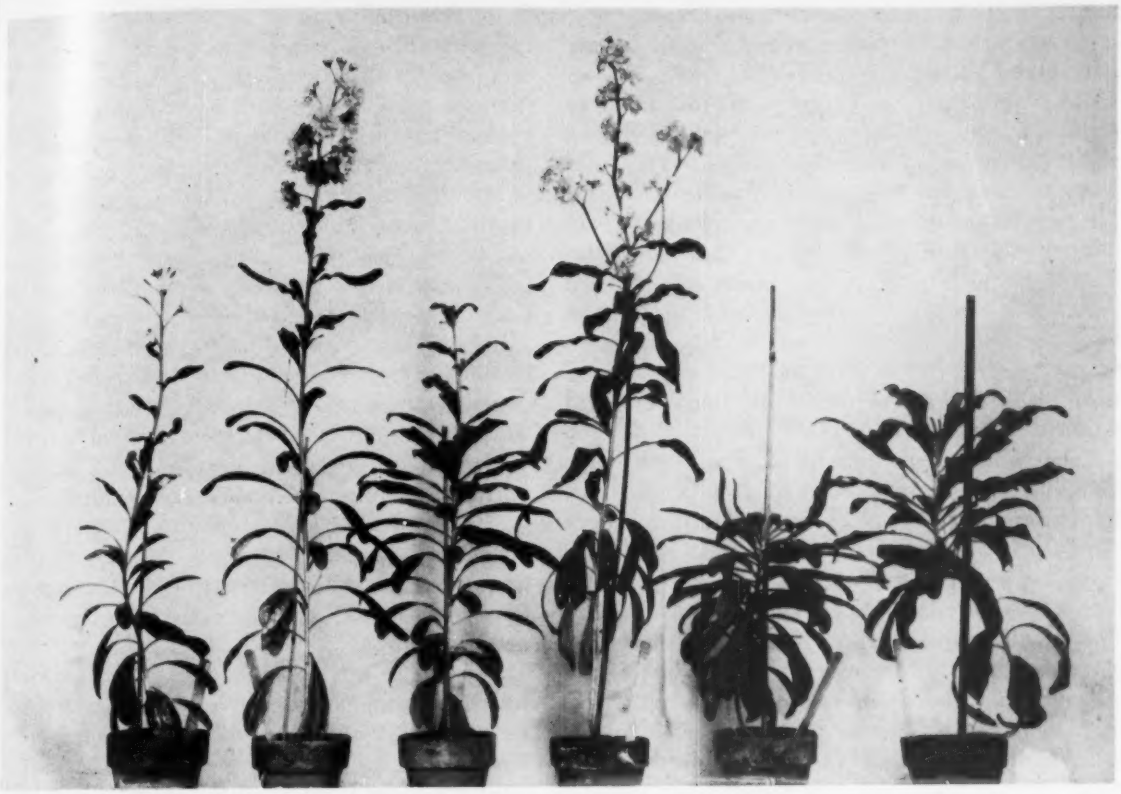

FIG. 1. Plants of stock (*Mathiola incana*) grown at different photoperiods and temperatures. Left to right: Short-day and 55° F.; long-day and 55° F.; short-day and 65° F.; long-day and 65° F.; short-day and 75° F.; long-day and 75°. Stock is considered a long-day plant, which it is at 65° F., but at a cooler temperature it flowers in both short and long days, and at a warmer temperature it remains vegetative in both short and long days.

first appearance of blossom primordia is not an index of when induction occurs, for induction occurs before the appearance of blossom primordia.

It has also been observed that the cambium becomes inactive in the roots of flowering annual plants. A general thickening of the cell walls follows.

Another method of controlling flowering has been demonstrated. As already mentioned, poinsettia responds as a short-day plant except at a temperature of 75° F. or above at night. It will blossom at this temperature, however, if a current of cool air is passed through a short chamber attached to the third or fourth internode from the stem tip (Fig. 4). The anatomical structure of the stem above the temperature chamber is characteristic of flowering stems. Applying a taut rubber band around the fourth or fifth internode from the stem tip also induces plants of poinsettia to flower (Fig. 5). Many workers have used the technique of grafting to induce vegetative plants to flower. This method, however, is applicable only to plants that blossom systemically, that is, plants such as *Xanthium* and soybean that initiate blossoms at nearly all nodes or growing points along the stem (Fig. 6). When a fruiting plant of *Xanthium* is grafted to a vegetative plant, the latter will be induced to flower. With plants such as *Salvia* and *Cosmos,* which produce flowers terminally only, grafting will not induce vegetative plants to flower (Fig. 6). Plants which blossom systemically and are induced to flower by the grafting technique show changes in the anatomical structure of the stem similar to those of other stems from flowering plants. From these results it appears that the transfer of the blossoming stimulus depends on the flowering habit of the plant. There is another difference between the terminal and systemic flowering plants in their response to environment. Plants that flower terminally, such as *Cosmos,* poinsettia, stock, and tobacco, readily revert to the vegetative state when placed in an environment inducive to vegetative growth. Systemic flowering plants such as morning glory, soybean, and *Xanthium* remain in the flowering state after having been returned to an environment inducive to vegetative growth.[4]

The flowering of plants that are indeterminate or day-neutral cannot be readily controlled by any

of these methods. Such procedures as regulation of water and nitrogen supply, however, can frequently control their flowering responses.

Since vegetative and flowering plants differ structurally, it has been of interest to observe whether they respond similarly to other environmental treatments. One of the treatments studied was the effect of boron deficiency on plants that are photoperiod-sensitive and those that are day-neutral. Plants of soybean var. Biloxi and Manchu grown in long days (where they remained vegetative), and without boron, showed typical deficiency symptoms: fragility of the leaves and petioles and necrosis of the apical meristem. Boron-deficient plants in short days, however, continued to develop in a nearly normal manner. Blossom primordia were apparent in all treatments in short days; long-day plants remained vegetative throughout the experiment. At the time of harvest, deficiency symptoms were very severe in long-day plants, whereas plants grown in short days appeared essentially normal. The anatomical structure of the stems of these plants revealed some interesting features. Plants grown without boron in long days showed an unusual type of anatomy in that the phloem region was about four times the width of the normal stem. This great difference was the result of increased number and size of phloem and ray cells resulting from abnormal cambial activity and some proliferation. Furthermore, considerable necrosis had occurred in these regions. The anatomy of the stems of plants grown with and without boron but in short days was not greatly different. Anatomical characteristics of fruiting stems, such as thickening of the walls of the vascular tissue and absence of cambial tissue, were apparent. Plants grown without boron were blossoming. There was slight enlargement of the phloem parenchyma and ray cells in short days, but they were much less conspicuous than those grown in long days without boron. The boron content of the leaf tissue in boron-deficient plants grown in short and long days was similar. In plants given one type of treatment (short days where plants flowered and fruited) the boron supply appeared to be adequate, but in a group given a different treatment severe boron deficiency symptoms occurred. Apparently induction, reduced cambial activity, and development of flower primordia were associated with the reduced need for boron.

Plants classified as indeterminate or day-neutral were also examined. Transverse sections of tomato stem showed the anatomical characteristics of flowering stems. Plants grown without boron displayed cellular abnormalities such as abnormal cambial activity, and enlargement and prolifera-

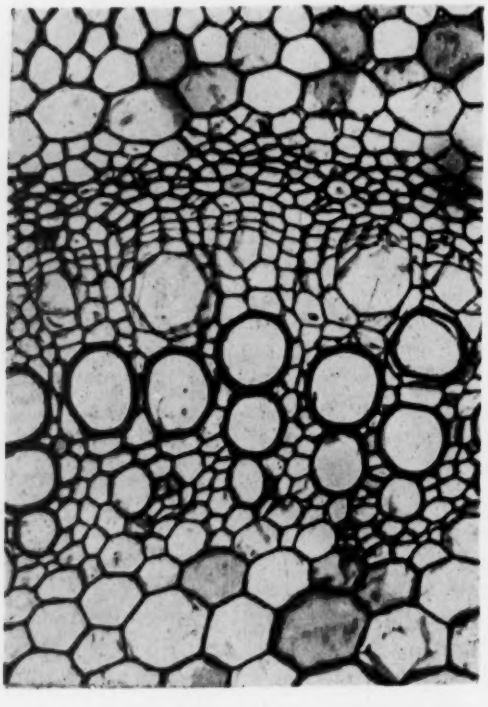

FIG. 2. Cross section in fourth internode from vegetative plant of *Salvia*. Cambium is active, and vascular tissue is being differentiated.

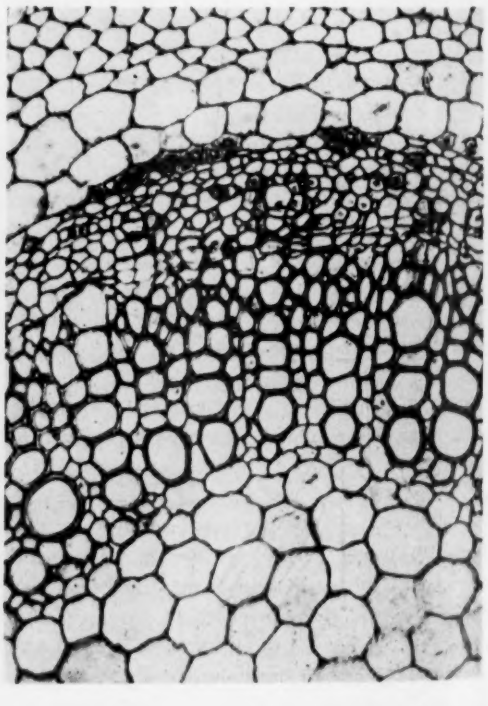

FIG. 3. Fourth internode from flowering plant of *Salvia*. Cambium has become less active, the cells are smaller, and vascular differentiation has been reduced.

FIG. 4. Left, plants of poinsettia that have been grafted together. Poinsettia produces flowers terminally only and is not induced to flower when grafted to a flowering plant. Right, a temperature chamber attached to the internode of a poinsettia plant through which a current of cool air has been drawn, thus producing flowers when grown at a warm night temperature of 75° F., with short days. This plant ordinarily remains vegetative in short days at this night temperature.

FIG. 5. Left, a taut rubber band wrapped around the stem also induced poinsettia plants to flower. Right, vegetative plant of poinsettia.

tion of cells in the phloem region resulting in a disruption of tissue which formed necrotic areas.

Soybean var. Pagoda also responds as an indeterminate variety, and comes to fruit very early in both short and long days. Anatomical characteristics of flowering stems were evident with all treatments, short and long days, and with and without a minus boron supply. Boron deficiency symptoms were practically lacking for this variety.[5]

These observations show that anatomical abnormalities are frequently evident when slight symptoms of deficiency are apparent externally. In plants such as *Xanthium,* buckwheat, and soybean var. Pagoda, in which cambial activity becomes inactive at an early stage of flower induction, symptoms of boron deficiency are repressed. In plants that come to flower slowly (tomato), the cambium remains active for a considerable length of time before flowering, so that the lack of boron has an opportunity to show its effects.

Hemp, poinsettia, soybean, and *Xanthium* ordinarily come to flower relatively early at a temperature of 65° F. When they were transferred to an environment where the dark period was held at a temperature of 75° F. and the light period at a temperature of 55° F.—that is, when the day and night temperatures were reversed—the plants soon became etiolated or produced new growth of a very pale green (Fig. 7). Nevertheless, the plants differentiated and developed blossom buds at almost the same time as plants grown with cool nights and warm days.[4] This indicates that blossom bud formation is apparently not related to greenness.

In addition to changes in the anatomical structure of the plants, other differences in flowering and nonflowering plants were observed. The environmental factors of photoperiod and temperature had little relation to the top-root ratio. This ratio was consistently different, however, for vegetative and flowering plants, and was uniformly larger when plants came into flower. Blossoming plants of a species have fewer roots in relation to the top at a given temperature than the vegetative ones.[6] There is a possibility that this factor could be related to the reduction in cambial activity resulting in decreased differentiation and increased thickening of the cell walls. In view of the transition in anatomical structure of the plants, an alteration in the transport of materials might also be possible.

In another experiment it was found that the area of leaves was also important in blossom bud initiation. The leaves of Wealthy apple spurs were measured, and the presence or absence of blossom buds was determined. By means of defoliation and defloration, the relation of leaf area to blossom initiation was determined, and it was found that for this variety of apple spurs with the two terminal leaves three square inches in area or more initiated blossom buds.

Many studies have been made of the effect of 2,4-dichlorophenoxyacetic acid on the killing of weeds. This chemical is effective for many plants; the results of its application are epinasty of the leaves and proliferation of certain types of cells in the stem and leaves, particularly the interfascicular cambium resulting in the formation of adventitious roots emerging through the cortex. Applying 2,4-D on fruiting plants of *Xanthium* gave very little stimulation of the internal tissues. In the fruiting state the cells have become thick-walled and are not stimulated to divide and proliferate as they do in the vegetative stem, where many of the cells are in a meristematic state and capable of division. In the

flowering plants there may be some meristematic cells at the nodal region that may be stimulated by this chemical. It is generally advisable to spray plants before flowering, so that overstimulation and proliferation of cells will occur later, resulting in the death of the plants. The effect of 2,4-D on monocotyledonous plants was the subject of another study. Many of these plants do not respond to the chemical, and the reason for this may be ascribed to the lack of a cambial region or similar meristem in the monocotyledons. Examinations of monocotyledonous plants have revealed that plants that have a rudimentary cambium, such as *Dracena, Antheri-cum,* and *Tradescantia,* will form adventitious roots when treated with 2,4-D.

Another difference between vegetative and flowering plants was in mineral residue when the microincineration technique was used. A greater concentration of mineral residue appeared to be present in flowering plants than in nonflowering plants. This suggests a redistribution of minerals or a change in mineral nutrition as the plants come to flower.[7]

It is important in approaching the subject of how plants flower that a study of flowering start at the time of induction, that is, before evidences of blossom primordia or changes in anatomical structure are apparent. It has been shown that regardless of the method used to induce flowering similar changes in structure occur. The flowering habit of the plants is a significant factor when attempting to control flowering. It might be wise to work with either vegetative or flowering plants when studying their nutrition and their response to growth substances, since flowering and nonflowering plants of some species react differently to nutritional treatments.

The next step in the study of flowering is to isolate and identify the growth substance that may induce flowering. This work is now in progress, and crystalline extract has been obtained from flowering plants which induces vegetative plants to come to flower very weakly and slowly. It also inhibits callus formation in wounds made in the stem. This extract, possibly a hormonelike substance, needs further investigation as to its properties, characteristics, and composition.[8]

FIG. 6. Left to right: Fruiting plant of *Xanthium* after being grafted to a flowering plant. When a branch of a vegetative specimen of *Xanthium* is placed in short days and another in long days, both branches fruit. *Xanthium* produces flowers at several growing points along the stem. *Klondyke Cosmos* (right) produces flowers terminally. When a flowering and a vegetative plant are grafted together, the flowering stimulus is not transmitted to the vegetative plant. Also, when one branch of a plant is placed in short days, and the other in long days, the flowering stimulus is not transmitted (extreme right).

FIG. 7. Left, plant of hemp grown in a warm day (75° F.) and cool night (55° F.). Branches in the long day do not flower even though branches on the same plant in a short day are in flower. Right, plant grown with warm nights (75° F.) and cool days (55° F.) showed etiolation of new growth. Flower buds were produced, however, in both normal and etiolated plants.

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A NORTHWEST STRONTIUM MINERAL DEPOSIT

A VEIN deposit of the two strontium minerals, strontianite ($SrCO_3$) and celestite ($SrSO_4$), occurs on the southern tip of Fidalgo Island, Puget Sound, Washington. The town of La Conner, Skagit County, connected by a bridge to the island, is only 1.5 miles from the outcrop of the vein on the almost clifflike shore line at this point of the island. The mineral deposit is most easily reached by boat from La Conner, by means of the narrow slough that separates the island from the mainland. The southern part of Fidalgo, except for 71 acres on the southeast tip, is Indian reservation. At present no road runs to this part of the island, but a resort road is contemplated to within one quarter of a mile of the mineral property. With little effort a low-tide beach road could be constructed for this quarter-mile distance to the mine tunnel portal.

Development and mining have so far been on a small scale. Access to the area has been by rowboat with outboard motor, and when mining, a small barge has been brought in at high tide and anchored near the mine. An air compressor has remained on the barge, and ore has been taken out

by barge, the distance to Seattle being about 65 miles.

Geology and Mineralogy of the Deposit

The vein of mixed strontium mineral has a nearly N–S strike and dips about 45° to the west. The vein matter is almost pure white as contrasted to the green and grey serpentinized dunite that constitutes the country rock. Undoubtedly there first existed a quite pure celestite ($SrSO_4$) deposit, which is being altered by ground waters containing carbonic acid to the less water soluble strontianite ($SrCO_3$). The celestite is well crystallized and bluish; the strontianite, white and finely crystalline. Apparently ground waters are precipitating some calcium carbonate, isomorphous with the strontium carbonate. A surface cut exposes the vein 150 feet from the brow of the cliff to the north in comparatively level ground. A tunnel 14 feet above Puget Sound water level is driven on the vein 50 feet to a fault and 40 feet straight beyond. The open-cut exposure is all beyond the point of tunnel fault (a possible cliff slide), so the continuity of the vein for a considerable length is verified. The height from the water's edge to the top of the cliff is about 90 feet; the width of the vein varies from 6 inches to 5 feet, with an average of a little less than 2 feet. Geologists have estimated that there are 10,000 tons of commercial strontium minerals above water level. Specimen pieces of celestite are almost 100 per cent $SrSO_4$, and strontianite specimens are nearly all over 95 per cent pure, calcium carbonate being the main impurity. Run-of-mine material, taking care to eliminate serpentine, runs 37 per cent celestite and 50 per cent strontianite, thus totaling 87 per cent strontium minerals, with calcium carbonate and serpentine constituting almost all the remaining 13 per cent.

Economic Aspects of the Northwest Deposit

Strontium nitrate gives a red color to flame, and consequently most red flare fusees or red military pyrotechnic mixtures contain strontium nitrate mixed with gunpowder. For this reason strontium minerals are on the government strategic-minerals list. Another use of strontium mineral is in the purification of caustic soda. This in itself may seem unimportant, but it is interesting to note here the far-reaching aspects of modern applied science. The iron content of commercial electrolytic caustic soda is about 40 parts per million, and it has been found that 300-mesh strontium sulfate or carbonate settling through a 50 per cent sodium hydroxide solution will adsorb most of this iron impurity. Sodium hydroxide used in the rayon industry must be of low iron content, or ultimate color stain of the fabric will develop; but more important yet is the fact that viscose (made from cellulose, carbon disulfide, and sodium hydroxide) with iron impurity in it has variable viscosity as it is forced

Surface cut along strike of strontium mineral vein.

Portal of mine tunnel just above Puget Sound tidewater level.

Outcrop as exposed in surface cut.

Specimen of $SrSO_4$-$SrCO_3$ rock. Bluish crystalline $SrSO_4$ is being altered to white $SrCO_3$.

through pinholes in platinum discs to form fibers. Thus variable fiber size results. Iron-free viscose, on the other hand, has a quite controllable viscose viscosity. It is, then, through the interrelationship of science and industry that an isolated strontium mineral deposit, because of its research-discovered capacity to adsorb the iron impurity from the much-used chemical, sodium hydroxide, contributes to better rayon. Other industries, such as the explosives industry, also make use of iron-free caustic. Celestite and strontianite, because of their high specific gravity, may be employed in place of barite to form a heavy mud for drilling deep oil wells, and the use of these minerals in ceramics is increasing. There are numerous miscellaneous uses for strontium compounds, but practically none for the metal.

The chemical industry for conversion of strontium mineral to strontium nitrate for flare and pyrotechnic use happens to be centered near Baltimore, Maryland. Although the Washington mineral deposit is at water's edge, and may easily be barged to Seattle for water or rail transport to the East coast, its cost still cannot compete with current ballast shipping rates for English or Mexican ore. Most sales of the mineral in recent years have been in powdered form to the Tacoma, Washington, electrochemical companies producing a pure caustic soda. These companies are branch plants of Hooker Electrochemical Company and Pennsylvania Salt Manufacturing Company.

Government stockpiling requirements are for a 90 per cent celestite, which would be a 43 per cent strontium metal material. Although the total strontium mineral percentage of the Northwest ore is slightly less than 90 per cent, it is important to note that the strontium metal percentage is higher in strontium carbonate than in strontium sulfate, so the 87 per cent ore runs 47.7 per cent strontium. Furthermore, all celestite ore has to be converted to carbonate or soluble compound before nitric acid treatment to produce nitrate. More than one half the Northwest strontium mineral is already carbonate. Accordingly, the Northwest strontium ore has a 4.7 per cent strontium content over stockpiling specifications and is already geochemically converted to about a 50 per cent extent to acid-soluble carbonate.

To date but a few 50-ton exploratory orders have been mined, but the mine is ready for production.

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PSYCHOLOGY OF HUMAN STARVATION AND NUTRITIONAL REHABILITATION

"Trop pâle et trop pauvre est notre langue pour pouvoir exprimer l'infini de notre malheur."—(Dr. Milejowski, the Ghetto of Warsaw, 1942.)

AS A result of the second world war the diet of uncounted millions was reduced in volume and deteriorated in quality. Close to 20 million human beings were subjected to semistarvation. A total of one million deaths due to starvation is not an exaggerated estimate of the grim harvest. The importance of adequate nutrition and the dire consequences of quantitative undernutrition and qualitative malnutrition were demonstrated in a cruelly convincing manner in many parts of the globe.

The detailed reports on medical aspects of semistarvation raging during the siege of Leningrad (1941–42); the clinical and experimental investigations of *Maladie de famine* among the Jews in the Warsaw Ghetto, with most of the investigators dead before the record of their labors was made available to the world; observations in a French mental hospital and a Belgian civilian prison dependent on the woefully inadequate official rations; the data on the effects of the 1944–45 famine in western Holland; experiences in camps for the Allied prisoners of war, both in Europe and in the Far East; research carried out in connection with the care of victims of the German concentration camps—all these, and other studies not mentioned, have contributed significantly to our knowledge of the biochemistry, physiology, and pathology of prolonged, severe undernutrition. Systematic, quantitative studies on the physical aspects of semistarvation were conspicuously rare. That such studies on the psychological aspects of starvation and dietary rehabilitation are nonexistent should cause little surprise. Standardized psychometric methods are hardly applicable under conditions of natural starvation.

Accounts of the personal aspects of semistarvation, of overt behavior, and of psychological changes experienced during starvation are by no means lacking. Unfortunately, the records of starved explorers and victims of famines and the

notes of relief workers are, for the most part, unsystematic and uncritical. Furthermore, privations and hardships of other than nutritional character are likely to complicate severely the physiological as well as psychological effects of starvation.

The practical importance and scientific relevance of systematic, quantitative information on the relationship between nutrition and psychological characteristics of starving man were recognized in planning the Minnesota starvation-rehabilitation experiment. The experimental work was carried out in 1944–46. Preliminary accounts of limited phases of the experiment, including behavior and personality,[1, 2, 3] have been published, and the full presentation of the experimental data, together with a survey of the world's literature, is in press.[4] Here we shall consider briefly the psychological manifestations, with especial reference to the levels of caloric intake during the period of nutritional rehabilitation.

Thirty-two conscientious objectors, twenty to thirty-three years of age, free of disease, served as volunteer subjects and resided at the laboratory for the duration of the experiment. There was a 12-week control period, a 24-week period of semistarvation, and a 12-week period of restricted refeeding. Twelve men remained for 8 additional weeks of unrestricted refeeding. Follow-up examinations were made on a part of the group up to one year after the end of starvation.

The average caloric intake, providing daily 3,492 calories during the last 3 weeks of the control period, was abruptly decreased to 1,658 calories on the first day of the semistarvation period. The grand mean daily intake during 24 weeks of semistarvation was 1,570 calories. The diet incorporated, as much as possible, the foods likely to be used in European famine areas. Whole wheat bread, potatoes, cereals, turnips, and cabbage were the major food items. Meat and dairy products were provided only in token amounts. The actual food intake was adjusted individually so as to allow the subject to follow closely a predetermined rate of weight loss. In individuals of normal weight for age and height the total weight loss was set at 25 per cent of the control value. During the first 12 weeks of the rehabilitation period the men were divided into four groups, with eight men in each group. The average intake of these four groups Z (Zero), L (Low), G (Good), and T (Top) was 2,378, 2,692, 3,123, and 3,392 calories, respectively.

The total course of body weight[4] is indicated in Figure 1. On the average, 36.8 per cent of the body weight lost during starvation was recovered after 12 weeks of refeeding; in the four caloric groups, the recovery amounted to 21.0, 29.6, 40.7, and 57.2 per cent. The weight continued to rise rapidly during the subsequent 8 weeks of unrestricted food intake and surpassed the control value. The body fat, as estimated from the specific gravity of the body, decreased dramatically during starvation. During rehabilitation it rose considerably above the original value. The changes in the buttocks and the abdomen were most striking in these respects, and during semistarvation the subjects were very uncomfortable when sitting in chairs without "padding." In later rehabilitation they were concerned about becoming flabby and pudgy. The typical appearance of the subjects at the different stages of the experiment is shown in Figure 2.

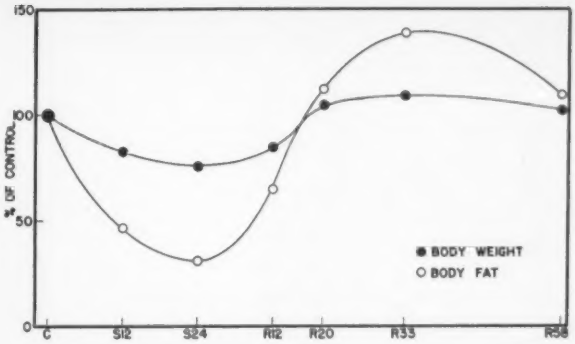
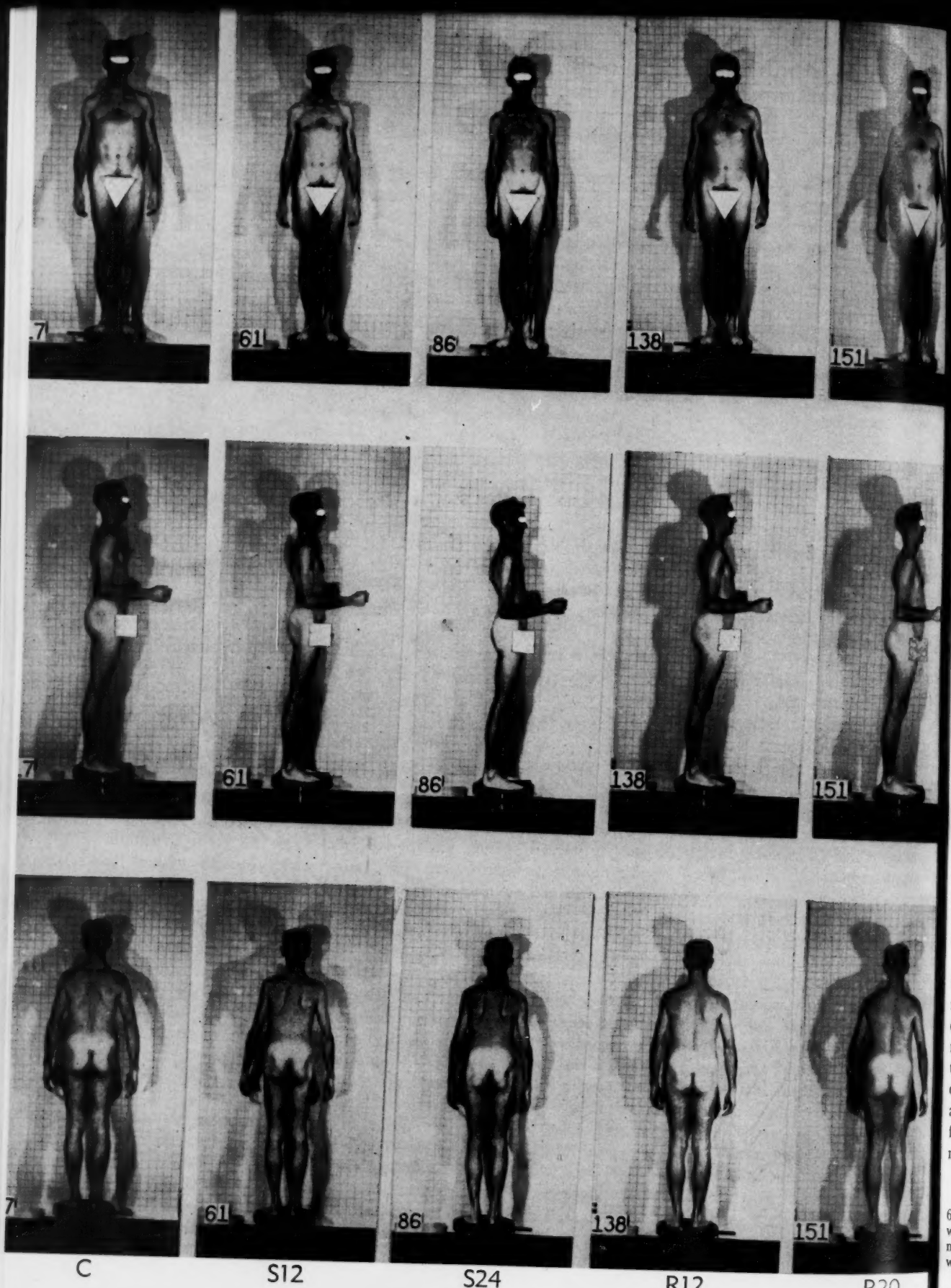


FIG. 1. Mean body weight and body fat, expressed as percentages of the control values, for thirty-two subjects at different stages of the Minnesota starvation-rehabilitation experiment. *C*, control; *S*, weeks of semistarvation; *R*, weeks of rehabilitation.

A variety of techniques was used for the study of the effects of semistarvation on behavior, intelligence, and personality. They ranged from intelligence and personality tests through ratings to purely descriptive material, provided by the experimenters' notes and the diaries kept by the subjects. Several sensory functions (visual acuity, flicker fusion frequency, auditory acuity) were measured, and motor performance was studied by means of a battery of tests of strength, speed, coordination, and endurance. This brief discussion will be limited to complaints and personality.

Various symptoms and complaints, noted in field reports, were rated by the subjects on a 6-point scale (0 = normal, 5 = extreme). At the end of the starvation period Tiredness had the highest average rating (3.5), followed by Appetite (3.1), Muscle Soreness (2.1), Irritability, Apathy, Sensitivity to Noise, and Hunger Pain (1.8). There were complaints of Loss of Ambition, Loss of Self-Discipline, Decrease in Mental Alertness, and Decrease in the Ability to Concentrate (−1.8 to −1.6),


7
61
86
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C
S12
S24
R12
R20

Moodiness (1.5), Dizziness (1.3), and Craving for Salt (0.8) associated with a large intake of fluid. After 12 weeks of rehabilitation a marked improvement in all symptoms was noted, the continued desire for more food and the feeling of tiredness being the most important semistarvation residues. At the end of another 8 weeks—i.e., after 20 weeks of refeeding—the subjects rated themselves as being close to the prestarvation status. By the thirty-third week of rehabilitation the recovery, as judged on the basis of self-ratings, was complete.

In addition to the detailed self-ratings, each man was rated as to the over-all deviation from his prestarvation status by a group of his close associates. The ratings were made by the subjects independently. The rating scale extended from 0 (no deterioration) to 5 (extreme deterioration). The average values of these deterioration ratings were 1.6 by midstarvation and 2.3 at the end of the starvation period, decreasing to 1.4 and 0.7 after 6 and 12 weeks of rehabilitation. In terms of the mean intersubject ratings, the recovery after 12 weeks of refeeding amounted to 70 per cent of the semistarvation deterioration. For the four caloric groups the recovery was estimated at 59, 67, 76, and 82 per cent, respectively.

Self-ratings of the three important "drives"—food, sex, and activity—were made at regular intervals. The concern with food and the felt need for food rose steadily during the 24 weeks of starvation, whereas there was a decrease in the activity drive and, more markedly, in the sex drive. The mean course of the changes in semistarvation and in the first 12 weeks of rehabilitation is charted, for the group of thirty-two subjects, in Figure 3. Qualitatively, the course of the changes conforms closely to the clinical impressions of the investigators. However, the *magnitude* of the deviations from the normal appears to have been underestimated by the subjects.

By the end of the semistarvation period, the sex feelings had become virtually extinguished. The number of "dates" dropped drastically. Nocturnal emissions and masturbation ceased or were greatly reduced in frequency. This was reported also to be true of the sex dreams. It may be noted that the excretion of 17-ketosteroids in the urine, expressed as mg of androsterone per 24 hours, decreased in five subjects from the normal level of 12.2 mg to 7.9 mg after 8 weeks of semistarvation and remained

FIG. 2. Appearance of subject No. 4. His weight was 62.0 kg during the control period (*C*), 47.4 kg after 24 weeks of semistarvation (*S*24), 57.7 kg after 12 weeks of nutritional rehabilitation (*R*12), and 66.4 kg after 20 weeks of rehabilitation (*R*20).

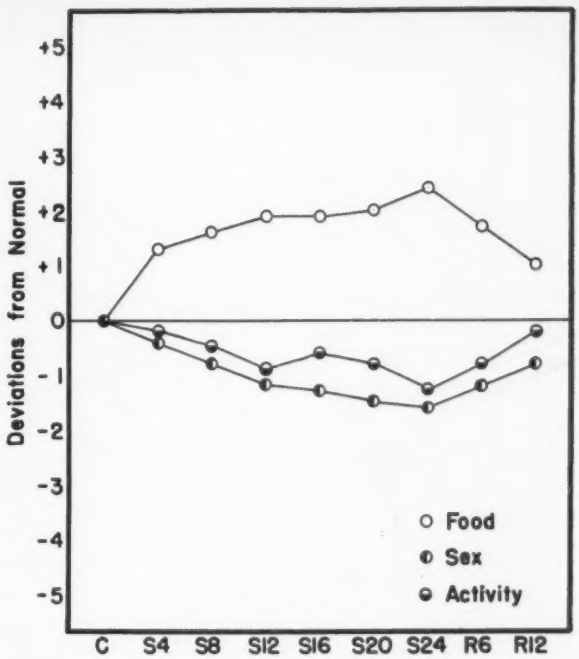

FIG. 3. Average self-ratings of the food, sex, and activity drives for thirty-two subjects during 24 weeks of semistarvation and 12 weeks of rehabilitation. (Normal, 0; Extreme, 5.)

at about the same level during the rest of the starvation period.[4] It returned to a near-normal value of 12.1 mg after 6 weeks of refeeding. The change in ketosteroid excretion, both during semistarvation and rehabilitation, was more rapid than the change in sex behavior and sex emotions.

Examination of the semen revealed profound changes.[4] Semen specimens were obtained from sixteen subjects after 24 weeks of starvation and 6, 11, and 20 weeks of rehabilitation. The mean volume of the ejaculate at the four examination periods was 1.6, 1.8, 2.4, and 4.2 cc (normal values, 2–6 cc). The longevity of the sperm, expressed as the hours for which the sperm remained motile, changed even more dramatically, with mean values of 5.4, 15.1, 16.6, and 33.0 hours (normal values, 24 plus).

The standardized personality tests and inventories used in the experiment included the Rorschach test, Rosenzweig's Picture-Frustration study, word association, the inventories of Guilford and Martin, and the Minnesota Multiphasic Personality Inventory. The latter proved to be particularly useful. It served for the determination of the degree of "normality" of the subjects at the start of the experiment and for the tracing of personality changes at different stages of semistarvation and rehabilitation. In the control period the scores were

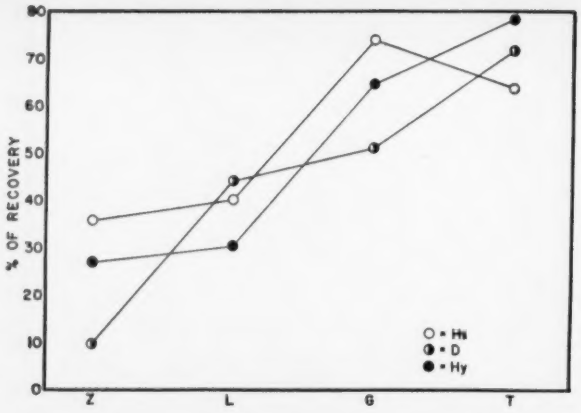

FIG. 4. Improvement in Hypochrondriasis (*Hs*), Depression (*D*), and Hysteria (*Hy*) scores on the Minnesota Multiphasic Personality Inventory after 12 weeks of controlled refeeding. The amount of recovery was expressed in reference to the deterioration suffered during 6 months of semistarvation. *Z, L, G,* and *T* refer to the four calorie levels of refeeding, representing a daily average intake of 2,378, 2,692, 3,123, and 3,392 calories.

within one standard deviation from the mean on all eight clinical scales of the inventory (normal average for the scales = 50, 1 S. D. = 10). During starvation there was a marked rise on the three scales constituting the "neurotic triad"—Hypochondriasis, Depression, and Hysteria—with mean increases of 7.3, 17.7, and 11.0 points, respectively. The changes were statistically highly significant, as well as clinically important.

The amount of recovery from the "semistarvation neurosis" tended to parallel rather closely the caloric intake (Fig. 4). The differentiation was particularly striking for the Depression scores. As may be seen from Figure 5, there was only small recovery in the lowest caloric group (Z) after 6 weeks of refeeding. In the subsequent 6 weeks there was actually a slight deterioration rather than further recovery in Group Z. For these subjects, with a weight recovery of 0.3 per cent after 6 weeks of refeeding and 21.0 per cent after 12 weeks of refeeding, the slow rehabilitation constituted in reality a continuation of the "stress."

At the end of an additional 8 weeks of refeeding, during which the food intake was not restricted,

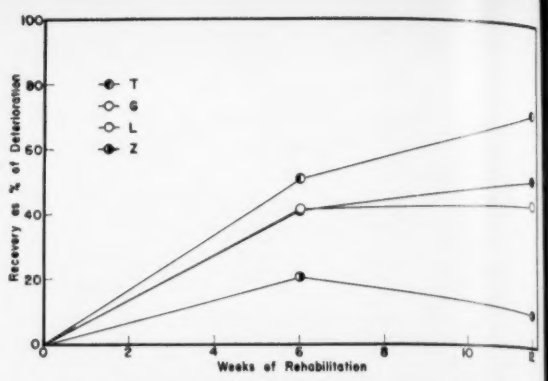

FIG. 5. Recovery in the Depression scores on the Minnesota Multiphasic Personality Inventory in the four caloric groups, *Z, L, G, T,* during the first 12 weeks of controlled refeeding.

there was still a difference between men who had been placed in the two lower and the two upper caloric groups during the 12 weeks of controlled refeeding. The mean recovery in the Depression score amounted to 43 and 80 per cent, respectively. As a part of the follow-up studies, twenty men were tested after 33 weeks of rehabilitation. At this time the mean values on all scales of the Minnesota Multiphasic Personality Inventory returned to normal (prestarvation) levels, indicating a complete recovery.

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BOOK REVIEWS

ORIGINS

The Splendour That Was Egypt. Margaret A. Murray. xxiii + 354 pp. Illus. $10.00. Philosophical Library. New York.

I DO not know exactly why I was asked to review this book, since I am not a specialist in Egyptology but primarily an Assyriologist. I may only guess that the choice was made in order to test the reaction of one of the general audience to whom this book is chiefly addressed. My reaction is negative. It might have been less so if I had started the book by reading its six main chapters, skipping the Introduction. But the facile, exaggerated, unbridled statements in the Introduction spoiled my appetite for the rest of the book. Here are some examples:

In the very first sentence of the Introduction we read about Egypt that "within the narrow limits of that country are preserved the origins of most (perhaps all) of our knowledge." Even considering the cumulative evidence of all the ancient civilizations—Mesopotamia, Egypt, Palestine, Syria, Crete, Anatolia, Iran, India, China—such a sweeping statement about the origin of our knowledge could not be taken seriously.

The statement that "the earliest known intelligible writing is the Egyptian" does not take account of the prevalent opinion held by such modern Egyptologists as Scharff, Stock, Frankfort, let alone the Assyriologists, that the Egyptian writing is several centuries younger than the Sumerian writing in Mesopotamia and that the former originated under the stimulus of the Mesopotamian civilization. In the same light must be judged the reference to the Egyptian invention of "the first actual writing materials;" in addition we fail to understand the use of the word "actual."

In the face of such names as Anaximander, Hecataeus, Eratosthenes, Hipparchus, Ptolemy, Herodotus, Xenophon, Polybius, and Strabo, the statement that the Egyptians' "acquaintance with geography makes the Greeks look like ignorant barbarians" is too preposterous for words.

One does not have to know Upper Egypt from personal experience, or be a trained climatologist, to feel that the claim that "the summer's heat in Upper Egypt . . . never falls below 105° during the twenty-four hours" is more than slightly exaggerated.

So much for the Introduction. The rest of the book is concerned more with facts than fancy interpretations, and the general reader, if not too sensitive, may get some understanding of the splendor that was Egypt. The book is well done typographically and sumptuously illustrated.

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The World and the Atom. (Rev. ed.) C. Møller and Ebbe Rasmussen. 192 pp. Illus. $2.50. Macmillan. New York.

THIS book was written by two Danish physicists who have been in close association with Professor Bohr. It was first published in Denmark in 1938, since when there have been several revised editions, the latest (January 1947) bringing the contents up to date, including the atomic bomb and further possibilities of obtaining atomic energy. The book under review is an English translation of the latest Danish edition.

The book begins with the last years of the nineteenth century, when physicists were generally of the opinion that physics was a completed science, in which no further important discoveries were to be expected. This state of mind was suddenly upset by the discovery of X-rays and of radioactivity, capped by Rutherford's discovery of the atomic nucleus. These new developments showed that the atom was no longer to be regarded as the smallest structural unit, and during the following quarter of a century there followed the discovery of subatomic particles, of which half a dozen are now known.

The second section of the book deals with the theoretical attempts that were made to construct an atom built up of these particles. Of these attempts the Bohr atom is still today outstanding. The third section describes the discovery of isotopes and the solution of the old problem of the alchemists—the artificial transmutation of elements. It also discusses fission, the atomic bomb, and the possibilities of atomic power.

The book as a whole is written in a style which nonspecialists will find easy reading. Mathematical formulas are reduced to a minimum and for the most part are placed as footnotes. The book can be recommended to the lay reader as giving an excellent picture of the subject.

There is another aspect of this book which should make it interesting to physicists. In discussing the Bohr atom, the authors mention the criticism with which this theory was met when it was first propounded. Several objections were made on the ground that this atom violated certain established laws of classical physics. For instance, according to Maxwell's laws, an electric charge revolving in a circular orbit will radiate energy in the form of electromagnetic waves, and will gradually spiral inward, whereas Bohr assumed that in his atom the energy of the revolving electron remains constant. To meet this objection Bohr took the ground that the laws of classical physics cannot be unquestioningly applied to the world of atoms, but that in the atom quite other laws may hold sway. This would mean that as we go down to atomic magnitudes either there is a

sharp boundary line between classical and atomic laws, or else that there is a gradual change.

Schrödinger's wave-atom (1926) is free from these objections that were made to the Bohr atom, but the authors point out that there are other objections to it from which the Bohr atom is free.

Now on page 69 of this book the authors attempt to calculate the radius of the orbits in the stationary states of the simplest of all atoms—that of hydrogen. They say:

> We find ourselves in the peculiar position of being apparently unable to make any calculation whatever, since we have nothing to start from. By accepting Bohr's postulates we have, indeed, broken down all the bridges behind us, and have altogether forsaken the firm foundations offered us by the classical mechanics. In order that we may go forward, we will make ourselves guilty for the time being of something that looks like the greatest of inconsistencies. We shall pretend that the laws of ordinary mechanics do hold good for a hydrogen atom . . .

Making their calculations, they obtain results which are satisfactory, and they say: "Now that we have begun to apply the laws of classical mechanics in spite of Bohr's postulates, we will cast all hesitation aside, and proceed to calculate with what velocity the electron revolves in these stationary orbits." This position, taken by two disciples of Bohr, is impressive.

Another interesting point discussed by the authors is Dirac's theoretical prediction of the positron, which a few years later was confirmed by the discovery of this particle. The authors state that Dirac's theory was at first regarded as fantastic, as his equations indicated the existence of electrons with negative kinetic energy, and therefore negative mass. Such electrons, if acted upon by a force, would move *against* the direction of the force, a mode of behavior which led a playful technician to give them the name of "donkey electrons."

To sum up the present state of subatomic theory as presented in this book, it appears that the opportunity is open for a new atomic model.

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The Origin of Medical Terms. Henry Alan Skinner. viii + 379 pp. $7.00. Williams & Wilkins. Baltimore.

NO TEACHER is in a more favorable situation to observe the problems confronting the individual embarking on the study of medicine than the teacher of anatomy. It is in this discipline that the terminologies and names of workers of days gone by constantly reappear. These early investigators were intimately familiar with the classics and encountered no difficulty in finding terms in Greek or Latin which could be applied to the various parts of the human body they were exploring. Whether one approves or not the fact remains that the modern followers of Aesculapius usually have had no training in either of these languages. The vocabulary of anatomy is particularly confusing because of the vestiges of the past which add to the problems of orientation and attrition.

An attempt has been made in the Skinner volume to provide a general reference book of standard medical terms, especially those likely to be encountered by the medical student as he enters upon the study of medicine. It is particularly directed toward the basic sciences of anatomy, physiology, biochemistry, and pathology. The choice of words has been heavily tinctured by the author's fields of interest. The biographies are in general those of the anatomists, chemists, and physicists. They are all well done. One does note the absence of such names as Neufeld, Domagk, Roux, Calmette, Fleming, Welch, Theobald Smith, Novy, and Park, which might suggest a second volume.

How far the author has succeeded in attaining his objective will remain for the students to determine. The book may be highly recommended.

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INDEFINABLES

Measuring our Universe. Oliver Justin Lee. x + 170 pp. Illus. $3.00. Ronald Press. New York.

IF THE reader is a *student* of Sir Isaac Newton, he perceives Dr. Oliver Justin Lee as an earnest seeker of the "ocean of truth," who is confused by the smooth pebbles and pretty shells and all the time afraid to leave the shores of tradition. He is frequently seen to dash toward the water only to halt as though blinded by the flaming sword and the Cherubims placed at the east of the Garden of Eden.

Measuring our Universe is a revision of *Beyond Yonder* in which the greatest revision occurs in the title and table of contents. Approximately 50 per cent of the text remains untouched.

It is with great care that Dr. Lee polishes pretty shells and smooth pebbles, giving them a warmth and glow of personality, but he seems always to drop them back into the sands of dogma. The distinction between truth and dogma is in reality so fine that unless one sees clearly at all times the criteria of distinction he is likely to be inveigled by the marvelous colors in the sands.

Every word one uses, if he is rational, is based upon the assumption that every other word is consistently defined and related to it, but the infinitude of possible words has caused man to assert that there must be a foundation of some absolute words and ideas which are indefinable and unprovable.

Distance is asserted to be one of these indefinables. This word haunts Dr. Lee, as it seems to haunt many other thinkers. It seems to have meaning, but as soon as one reflects upon its "scientific usage" he finds a mere pretty shell of thought. Dr. Lee is forever trying to polish this shell, succeeding only in confusing the idea with the method by which the idea is measured. This is forgivable, as the modern mathematicians now define differentials in this manner.

To complete the idea of *Measuring our Universe* one should reflect upon the true meaning of relativity and

thus help place Dr. Lee in the "ocean." Every material body is a center of a field of force. To find what this is, let us reflect on what exists between us and yonder wall. First there are oxygen, nitrogen, carbon dioxide, and some other gases. Let us remove all gases. What remains? There are three names: ether, gravitational field, and the field of inertia. Reflect now upon the would-be results of removing these one at a time and learn that the three names denote merely different aspects of the same essence. Einstein's space-time is therefore the field of force used in defining matter; and distance is the geometrical abstraction obtained when time is not relevant.

To deny this is to force oneself to accept the shells of dogma, but to accept it one finds *Measuring our Universe* a worth-while experience, as it relates in humanizing style the care used and required to give polish to the data man has acquired and now calls scientific knowledge.

There is one thought given on page 51 of *Beyond Yonder* which I consider unfortunately omitted from *Measuring our Universe*: "They have not been forgotten. They were never known."

A *student* of Newton lives by the following quotations:

> I do not know what I may appear to the world; but to myself I seem to have been only like a boy playing on the seashore, and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me.
>
> The inverse square law contradicts all possible physical definition for the word "force" and therefore is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it.
>
> The inverse-square law is an as-if-law.

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In Search of the Miraculous. P. D. Ouspensky. xii + 399 pp. $5.00. Harcourt, Brace. New York.

PETER DEMIANOVICH OUSPENSKY (1878–1947) was born in Moscow and died near London. He wrote *The Fourth Dimension* (1909), *Tertium Organum* (1912), and *A New Model of the Universe* (1914). According to his publisher, "He combines the logic of a mathematician with the vision of a mystic." This, his posthumous work, consists primarily of his record of the conversations he had with his teacher, the mystic Gurdjieff.

It is, perhaps, unfair to the author to evaluate his book as a scientific contribution or to apply to it the mundane standards of scientific criticism. The author is functioning in a field where evidence and experimentation are lacking and where truth is established by the word of the master. Of great importance in the demonstration of truth was the master's facial expression, whether he smiled or frowned as he spoke—of minor importance was his plausibility. It might be well for someone to make an objective study of mystics, complete with questionnaires, so that we could have proper coordinates for evaluating them. The reviewer is working without proper guides and is certainly not competent to pass judgment on the "essences" and "astral bodies" in what is to him an alien world.

Ouspensky obviously did not live in the cosmos which scientists investigate, although at times he seemed to be making efforts to get in touch with it, to understand human problems and to solve them. He helped Gurdjieff organize the Institute for the Harmonious Development of Man, where "the subjects of study were gymnastics of all kinds (rhythmical, medicinal and others). Exercises for the development of will, memory, attention, hearing, thinking, emotion, instinct and so on." Here in a friendly atmosphere "a Russian doctor was curing tuberculosis by treating the spleen with X-rays," so it seems that the contact was never really established.

Ouspensky seemed also to be trying at times to deal with the chemical elements, as is shown by the following quotation from page 324:

> In our conversations about this diagram we very soon agreed to take "angels" as planets and "archangels" as suns. Many other things gradually became clear to us. But what used to confuse us a great deal was the appearance of "hydrogen" 6144 which was absent altogether in the previous scale of "hydrogens" in the third scale which ended with "hydrogen" 3072. At the same time, G. insisted that the enumeration of "hydrogens" had been taken according to the third scale.
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> A long time afterwards I asked him what this meant.
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> "It is an incomplete 'hydrogen,'" he said. "A 'hydrogen' without the Holy Ghost. It belongs to the same, that is to the third scale, but it is unfinished.
>
> "Each complete 'hydrogen' is composed of 'carbon,' 'oxygen,' and 'nitrogen.' Now take the last 'hydrogen' of the third scale, 'hydrogen' 3072. This 'hydrogen' is composed of 'carbon' 512, 'oxygen' 1536, and 'nitrogen' 1024.
>
> "Now further: 'Nitrogen' becomes 'carbon' for the next triad, but there is no 'oxygen' for it and no 'nitrogen.' Therefore by condensation it becomes itself 'hydrogen' 6144, but *it is a dead hydrogen* without any possibility of passing into anything further, a 'hydrogen' without the Holy Ghost."

There are nearly four hundred pages whose content and tone do not differ very greatly from the above. The author must have found some enjoyment in this universe of his own creation, for he spent his life developing it and in seeking converts and companions. The reviewer on the other hand finds it boring. Occasionally there is a rewarding passage such as, "Only a man who fully realizes the difficulty of awakening can understand the necessity of long and hard work in order to awake. . . . There is also the possibility of being awakened by mechanical means. A man may be awakened by an alarm clock. But the trouble is that a man gets accustomed to an alarm clock far too quickly." By comparison such passages seem wise and profound. But, all in all, the book seems unrealistic.

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MACHINES AND MEN

I WAS very much interested in the article entitled "The Morality of the Machine," written by Mr. Elgin Williams.

As a businessman who has studied many of these problems for years in an attempt to discover new avenues of thought that would result in social betterment, I would like to point out that Mr. Williams proposes a solution which is very frequently evolved by students who are engaged in scientific and academic work. This approach involves the application of the objective actions and viewpoints of science to social problems. This approach might work if the world were made up of people who had the objective personalities of the writers and the fine social consciousness which moves them to propose such solutions. Unfortunately, the percentage of people who are like this in the world is extremely small, and any real solution must recognize normal human nature and the forces that are developed by that nature. It is often the good intentions of idealists that plunge nations into the blood baths of the French Revolution, the Communist regime, and other actions of this kind. Some of the points that are forgotten by many writers, including Mr. Williams, are:

1. That the machines are operated by men, and the planning is done by men who do not have a social conscience but respond to many kinds of incentive such as wealth, power, and social prestige; and that if a mathematically exact system were worked out for manufacture and distribution it would soon be upset because people would not be satisfied unless they could show themselves to be relatively better than others.

2. It is usually assumed that what exists today in the way of supply, demand, output, etc., will continue; and yet this never happens. In a dynamic situation of moving forces, there is going to be a change in demand going on constantly. If it is automobiles, they will improve or become poorer. If it is groceries, the fresh seasonal fruits and vegetables will first be canned, and then frozen, and perhaps later delivered in the form of a juice.

3. These planners usually fail to recognize that people who are competent in politics will inevitably run any kind of system, and we know that politicians can and will sell their influence for something that they consider to be useful. This is done in America, and it is done in Russia, and it is done in every other country in the world.

4. The planners can all work out apparently foolproof systems of rationing the output of the machines and fields, but they never recognize that men will find ways to develop black markets and upset this careful distribution, no matter how drastic the penalties may be.

5. Such planners also fail to consider how they are going to distribute goods of relative difference in value. Who will get the Cadillacs and who will get the Fords and Chevrolets and Plymouths? And who will get the little Austins or Crosleys? Who will get a new car this year and who will have to put up with an old one? These differences have to be taken care of. They are now adjusted in America and some other countries by price and the desire of different people to spend for one thing instead of for another, whereas in Russia they are determined by the rank of the individual in the hierarchy of the government.

I think that more attention should be given in our literature to the factors which influence people to work, to save, or to spend, to do an outstanding job or to do as little as possible. Only when we take these into account can we then consider methods of channeling such forces so that they produce the greatest social good.

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SINCEREST congratulations on three magnificent articles in the February 1950 issue! (1) On Elgin Williams' masterpiece of "The Morality of the Machine." This article by itself is worth the cost of membership in the AAAS for ten years. It should be made elective-required reading for every college and university student in America (in the world). I am going to write Williams to this effect. (2) On Dr. Stevens' article, "The Moral Obligation to be Intelligible"—another splendid and all too much needed contribution. I don't suppose it would do any good to make all Ph.D. candidates read it as required reading—their supervisors would have to read it first; and I doubt if they would be able to understand the fact that the article is about THEM! (3) Professor Benjamin's excellent review of Lilienthal's *This I Do Believe*. Again: congratulations!

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